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ARTICLE XXV.

THE REMOTE CAUSE OF SCROFULOUS FORMS OF DISEASE.

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MR. EDITOR:—The very favorable reception of my last contribution, as indicated by the many letters I have received in relation to it, and that too, immediately upon its reception, encourages me to send you another, which has some affinity with the last and, probably, of equal moment.

The ignorance of our profession, of the remote cause of scrofulous forms of disease, has for centuries been denominated the *approbrium medicorum*. But, sir, it can no longer be preferred, because I have discovered it; and that I have, I am just as confident as I am of any other proposition in the profession; and now I submit it to the profession, hoping that it may command the most searching observation. When scrofulous forms of disease are met with, they are, to the extent of my observation, attributed to an hereditary diathesis.

I am entirely certain that not less than ninety-five per centum of our scrofulous forms of disease are entirely independent of any such cause. I grant, however, that the scrofulous diathesis is hereditary, but I am not prepared to admit that it occasionally skips a generation and reappears. I grant, how-

ever, that a generation may escape scrofulous forms of disease, but it is because it escapes adequate cause of excitement; but the diathesis continues to be transmitted as long as it exists, while the certain index of its existence is not generally known. A feeble endowment of the motory centre is, I hold, an invariable index of the scrofulous diathesis; and this centre is in the cerebellum, midway between the mesial line and the mastoid processes, respectively.

Within a recent period, comparatively, considerable has been written in relation to the remote cause of scrofulous forms of disease. Dr. CUMIN, in Cyclopædia of Practical Medicine, article Scrofula, says:—"In considering the causes of a disease so deeply rooted in the constitution, as scrofula is universally acknowledged to be, it is necessary to direct our attention to circumstances very remote in the history of its subjects. The foundation of a scrofulous habit is frequently laid during the fœtal state, by the transmission of that peculiar organization of the frame from parents who themselves possess it." The Dr. was right in going far back in the history of scrofulous subjects to seek the cause, but he did not go back sufficiently far,—he only went back far enough to find a proximate cause, but not the remote one,—he did not find the remote cause of the first case that announced the dawn of civilization. The diathesis must have first been produced before it could have become hereditary.

Again, he says, "a cold and variable climate, like those of Great Britain and Holland, is particularly favorable of scrofula; in proof of which, it is sufficient to adduce the great prevalence of the malady in both of these countries." Concede the verity of this statement, and nothing more is gained than that these countries are well adapted to the development of a scrofulous diathesis into a scrofulous disease,—the cause of the diathesis is still undiscovered. Prof. EBERLE, in his Practice, by MCCLELLAND, p. 754, attributes the scrofulous diathesis to, 1. Climate and atmospheric influences; 2. To impure and confined air of cities; 3. To deficient and unwholesome food; and, 4. To various forms of disease. All these may be very efficient in

exciting into activity a pre-existing diathesis, but we have had no evidence, that can be regarded as satisfactory, that either or all of them can produce the scrofulous diathesis. Scrofula obtains greatly in Virginia, where the climate is genial and one of the best; and the people have always been distinguished for being good and generous livers. Scrofula also abounds in Massachusetts. The climate, it is true, is cold but steady; and no people are, probably, more comfortable livers, generally. Perhaps, no country furnishes more squalid poverty than Ireland, and yet the Irish are less scrofulous than their English neighbors. Scrofula is by no means common to our wild tribes of Indians, and yet they are very little more protected against atmospherical vicissitudes than are the forest animals, and during much of their time they are stinted in food, and the little they have is of the poorest quality. I have known entire families of Indians, women and children, to live through the winter exclusively on bitter acorns. Scrofula is also almost unknown to our frontier people, and very generally they are but poorly fed, clothed, or sheltered. If the scrofulous diathesis obtained with either them or the Indians, scrofula would equally abound, because they are constantly exposed to the most adequate causes of excitement.

Prof. WOOD, in his Practice, Vol. I., p. 114, has taken a more correct view of this subject than has any one else whom I have consulted. After alluding to the usually assigned causes for the scrofulous diathesis, particularly those above-named, he says, "There are many individuals upon whom all those causes may be made to operate, and so intensely too, as even to produce fatal effects, without giving rise to this particular disease. Indeed, it is probable that the great majority of mankind might perish under these circumstances and give no sign of the disease. There is something more, therefore, than mere debility. There is some inherent peculiarity of the organization, generally derived from the parents, which serves as the basis of the disease. The other causes are, in general, merely exciting. They no doubt induce the disease, when it might otherwise never have been developed; but they are, generally, incapable of producing

it, unless in subjects having some innate disposition towards it. In the great majority of fatal cases, the original and essential cause will, probably, be found to be an inherited peculiarity of the organization."

Prof. WOOD has very candidly led us to the inference, that of the cause of the scrofulous diathesis he knows nothing, but entertains the opinion that it is derived from the progenitors. Dr. CUMIN was of the same opinion, with this difference, he thought it was derived from such parents as had themselves the diathesis. Prof. WOOD does not specify any condition of the parents, hence, we are at liberty to infer that it may be derived from such as are normally organic and physiological; and this must be conceded to be the fact before we can reasonably expect to find the remote cause of the first case of scrofula that ever obtained in our race, and this is exceedingly desirable, because that remote cause may still be operating, and, possibly, with more power and frequency than ever before, and that this is the fact I doubt not; and it must be discovered before we can hope to emancipate our race from scrofulous forms of disease.

I am as confident as I am of any physiological truth, that the remote cause of all truly scrofulous forms of disease originates in a physiological incompatibility of progenitors. Dr. CUMIN, in searching for the cause of the scrofulous diathesis, went back to the fœtus; he found a cause, but it was not *the* cause. The cause of the scrofulous diathesis had also a cause, and I have discovered it to be, almost exclusively, civilization. To a very limited extent it is incidental to primitive humanity, in some of its relations, hence, scrofula, to a limited extent, obtains with our primitive people, the Indians. When I discovered it to be a fact, that the most sound and physiological of our respective sexes, in marriage, are frequently so physiologically incompatible as to entail a scrofulous diathesis on their children, the fact, *a priori*, seemed to me to be unreasonable and unphysiological, and, hence, I was greatly perplexed. But having ascertained the fact beyond the possibility of a doubt; I sought its rationale, and have most satisfactorily obtained it.

Ninety-nine per centum of our incompatible marriages are occasioned by the lymphatic and the encephalic temperaments, and a proper understanding of their origin, respectively, most thoroughly illuminates the subject. The lymphatic and the encephalic temperaments obtain only under the influences of civilization, they do not obtain with primitive people. One exception may be cited against me, but it is an error. Some physiologist, name not remembered, represents the Esquimaux people as being lymphatic, and they are primitive. When I saw this statement, I was strongly persuaded that it was an error; and having since obtained five of their crania, I find them to be bilious. They are an obese people; indeed, obesity is an essential condition of animal life in that climate,—a lymphatic person could neither originate nor live there.

I assume wealth to have originated with civilization, because the fact is universally conceded; wealth induces relaxation from toil, and many varieties of personal indulgence, and these enfeeble the physiological energies, and a lymphatic repletion is the consequence. Primitive and frontier people have to be in constant exertion to obtain the means of subsistance and, therefore, not permitted to become lymphatic. About our drinking-saloons and other haunts of idleness, people may be constantly seen who are rapidly becoming lymphatic; and, when this repletion is induced, a lymphatic diathesis becomes entailed. Care, responsibility, and mental activity, generally, are about as exclusively incidental to civilization as wealth is; and, thus, the cerebrum is developed to the neglect of the cerebellum; and, hence, a physiological debility as certainly eventuates as with the lymphatic condition. The constitution thus induced is the encephalic. Any observing physician may witness the constant development of the lymphatic and encephalic temperaments amongst his acquaintances.

The Hollanders are notoriously scrofulous and as notoriously thrifty, and, consequently, the lymphatic and encephalic temperaments are so exceedingly diffused amongst them that I think it very probable that not even one per centum of their marriages is physiological, and, hence, in about the same pro-

portion their people have a scrofulous diathesis; and as their climate is exceedingly favorable to develope the diathesis into scrofulous forms of disease, we can now understand why it is the Hollanders are so scrofulous. The great commercial prosperity of the English people and their climate, are rapidly rendering them as scrofulous as the Hollanders. Indeed, all the prosperous states of Europe are suffering greatly from the scourge of scrofula.

Such has been the extraordinary prosperity of our people for thirty-five years preceding our Civil War that the lymphatic and encephalic temperaments have, in that time, not only been increased but wonderfully disseminated amongst our people. Having been an active observer in this relation for forty years, I know that about which I write. As early as 1830, it was a novelty to see any one who participated in the encephalic temperament, but now it obtains in very nearly a-third of society, particularly in our cities. At the time above mentioned, the number was small who had a lymphatic repletion of the body, but now they are as plenty as blackberries in July; and the result of this great dissemination of the lymphatic encephalic constitutions amongst our people, is, that our physiological marriages are reduced to two-sevenths. Every physician of my age is aware of the fact, that scrofulous forms of disease have astonishingly increased in the preceding thirty years. Idiocy and imbecility are two of the results of incompatible marriage; and before 1830 there was not, I think, in our whole country a single asylum for the care of such children; but now almost every State has such an institution, and some two or more. This is significant. Some aged physicians think that all forms of disease are now less manageable than they were thirty years ago. This, I doubt not, is true, but it has not been because of any essential change in the abstract character of disease, but because the constitution of our people has become more or less depraved by the scrofulous diathesis, and is, therefore, less able than formerly to contend with disease. Physicians are now heard to say, that no skill can cure the children of some people when they become sick. This is true; but whose

children are these? I answer, confidently, they are the strumous apologies for children of incompatible progenitors. Some physicians, in Cincinnati, form their prognosis as soon as they see the parents. If they are compatible, the child can be cured; but, if otherwise, the child will die. All physicians may acquire this ability by the study of my Science of Physiological Marriage, as epitomized in the May and June Nos. of the *Examiner*.

Before the dawn of civilization, man's native instincts were sufficient to guide his matrimonial relations,—as they still are with uncivilized people; but the introduction, by civilization, of two constitutional conditions has placed the marriage relation beyond the pale of his instincts. Civilization has made the discovery of many sciences essential to man in his civilized state, and among them is the new science of physiological marriage; and it must be conceded that it is the most essential science ever announced for the consideration of man; and, if he neglect it, the premature extinction of his race is just as certain as it is that the sun rises in the east and will continue to do so, for the natural laws govern both.

Our Civil War is doing much to mitigate the scourge of scrofula, particularly in the Confederate States, but more particularly for the black population; for, before the Rebellion, slave property was depreciating, because of the prevalence of the scrofulous diathesis. As paradoxical as it may seem, it is nevertheless true, that war, pestilence, and famine are the great conservatives of our race, and, when they shall be no more, our race will soon be no more.

I am strongly inclined to believe that every physician in our country, who has had for a few years a tolerable practice, must remember enough of his observations to incline him to think it probable that the cause of the scrofulous diathesis is physiologically incompatible marriage. Consanguine marriages have been thought to be a source of the scrofulous diathesis; but this opinion I am confident is entitled no more consideration than any other prejudice, for as frequently as I have found consanguine parties to be temperamentally compatible, so fre-

quently have I found their children all right and without any distinction in relation consanguinity. When I find married parties to be temperamentally incompatible, I find the children with a scrofulous diathesis.

In concluding this thesis, I beg leave to cite one case as being illustrative of the science of physiological marriage and also of the remote cause of scrofulous forms of disease:—

A year or so since, one of the most able and distinguished theologians known to the Christian church of this or any other country called on me, introduced himself, and said:—"Are you Prof. POWELL?" I am, sir. "I have learned from some friends that you have made a highly interesting discovery in human physiology, and, as I have always been much interested in human physiology, I have called to get you to explain it to me, as my friends could not do it satisfactorily, provided it will be agreeable to you?" It will afford me pleasure, sir, please be seated. I explained to him the character of the physiological incompatibility that frequently happens between the most sound and physiological parties of our respective sexes in marriage, and the consequences. I found him by the by to be an extensively informed physiologist, and much regret that propriety forbids the promulgation of his name.

Upon the conclusion of my explanation, he said, "Suppose you see one of the parties to a marriage and have a description of the other, can you then infer any thing of their children?" I can, sir, if the description be correct. "Then, sir, I will give you a description of my wife." He described her, and said, "what do you suppose her temperament to be?" Bilious lymphatic, with a considerable predominance of the bilious. "Just so, sir, I think you are right; that has always been my opinion." "What is my temperament?" Sanguine bilious encephalic, with a strong predominance of the sanguine and bilious elements. "Now, if you please, your opinion of our children?" I think, sir, you have had a numerous progeny; that they, respectively, lived to adult age with a fair promise of age and usefulness; but, sir, before the age of thirty years they, respectively, died of phthisis. He sprang to his feet, exclaiming "stop, sir! stop,

sir! stop!" What, sir. "I wish to inform you that consumption was never an heirloom in my ancestry nor in my wife's, for I have the history of both for many generations in Europe." Having had no information in relation to your ancestors, respectively, I assumed your statement as being true when I gave my opinion of your children; nevertheless, sir, I may have erred, but, if I have, it was because of some misapprehension of your respective constitutions; but, not being conscious of any such misapprehension, I have no modification of my opinion to offer; and, if you are as old as I think you are, your children are all dead, and you know the fact and also the affection of which they died. "What has my age to do with it?" Nothing, sir, more than I have stated,—you have been permitted to know all the facts. "But, Professor, I conceive that there is a very formidable difficulty in the way of your opinion." Name it, sir? "I have been a physiological reader all my life, and have thus learned that when both parties to a marriage have good health and sound constitutions, their children will also; this I understand to be a settled doctrine in physiology. What are you to do with all this authority against your opinion? For my wife and I thoroughly fill the conditions; for a more sound and healthy couple than we are, and always have been, do not live in this or any other country." The authority you have named, sir, is not in my way, and as you have been extensively a physiological reader, you must be aware that I am the only one who has taught that the respective sexes of our species, however sound they may be, are frequently so incompatible, constitutionally, as to entail a scrofulous diathesis on their children. It is to such an incompatibility between you and your wife that I attribute your bereavements. "To the extent of my reading, you are the first and only one who has taught this doctrine, and you are a master of it in all its relations."

"I will give you the facts: my wife brought me just one dozen children, and all of them lived to adult age, and some of them married and had children, but, sir, neither of them lived to the age of thirty years,—all died of phthisis, and I am now a childless old man. But, sir, I confess myself actually unable

to comprehend how, on a subject of this kind, you can arrive at such a discriminating precision as you have manifested in relation to my family." To learn that I have investigated this subject sixteen years will, probably, help you. "No, sir, if you had investigated sixteen hundred years the difficulty would not be materially reduced, for it is neither chemistry nor mathematics. I can only regard your decisions as ultimate manifestations of the human mind. That you have made a discovery I cannot doubt, and one too that will astonish all the world. I will not say that it is the greatest discovery ever made, but, sir, it is the most important, because it most directly concerns the perpetuity of our race, which is confessedly the most important interest of humanity. You must prepare yourself for much incredulity, for this discovery will have more to contend with than any other ever had, because the subject has been thought to be the most inscrutable. It seems to me that your discovery of sexual incompatibility reveals another of no less brilliancy, and of which, possibly, you may not have thought. It is this: it reveals the cause of the scrofulous diathesis. As I was confident that such a diathesis was not in either my wife nor myself, I felt confident that I would balk you when you said, my children would die of phthisis, but I was mistaken. I have presented the facts of my family to many eminent physicians in both this country and Europe, but you alone have consistantly explained them; and you have given the only explanation that can be given, I think. The cause of the scrofulous diathesis has engaged the first minds of your profession for more than two thousand years, and that you have discovered it, my own case is as strong proof as can be had."

ARTICLE XXVI.

ERYSIPELAS, DIPHTHERIA, AND SCARLATINA.

By A. FISHER, M.D., of Chicago, Ill.

Sanitary Report read before the Chicago Medical Society, August 4th.

In reporting the sanitary condition of the South Division for the past three months, there is little to say. As far as I can learn, there has been no epidemic nor any unusual amount of disease of any kind. On the contrary, I think the city may now be regarded as uncommonly healthy, with fewer cases of bowel-complaints of children than we generally have at this season of the year, at least I find it so in my practice, and the physicians with whom I have conversed on the subject say, they have not had as many cases of the kind this summer as usual; besides, what few cases I have had have been very easily controlled, without the disposition to relapse, which is so common some seasons. However, within a short time I have had two cases of acute enteritis and one case of acute gastro-enteritis of adult females, which were very severe. The disease was controlled, in all of them, in a few days by cupping and leeching freely, with applications of hot fomentations of hops applied over the stomach and bowels, and continued for two or three days, renewing them as often as they became cold, together with ¼ gr. doses of calomel rubbed up with sugar and given every hour, and continued until the vomiting and great thirst was relieved and the secretions restored. I mention these cases because they are so very rare in my practice that I do not remember of having a well-marked case of the kind before for the past year, and to ascertain whether other members of the Society have met with many such cases this summer or whether they were accidental.

Typhoid fever has not prevailed to any extent during the summer, to my knowledge; and I cannot learn that bilious, remittent, or intermittent fever has been very prevalent, though, as usual, there are cases of the kind. There has been more or

less whooping-cough during the summer, perhaps more than usual, but not of a serious character.

I have not seen or heard of a case of measles this summer, still there may and, probably, have been cases which have not come to my knowledge.

The variola and varioloid which was so prevalent here in the winter and spring has nearly subsided, though I understand there has been quite a number of cases during the summer, and even now a case occasionally occurs, showing that the virus still remains among us. When we find many children, and occasionally an adult, who has not been properly vaccinated, it is not strange that the disease should continue to be propagated in a city like this. The wonder is, that it should ever be eradicated so long as there are so many who have not been protected by vaccination. In my opinion, there should be some way devised that every adult and child, over four or six months old, should in some way be compelled to be properly vaccinated. In that way only can we be sure that the pestilence will be entirely eradicated and kept from the city.

Erysipelas, scarlatina, and diphtheria, which prevailed to some extent last spring, I believe have nearly subsided, although a sporadic case occasionally occurs this summer. In the spring, quite a number of cases of erysipelas occurred in my practice, and among them four very interesting cases in one family, which I intended to report long ago, but as this is my first Sanitary Report, I will give a synopsis of them now, to show the intimate relation between diphtheria, scarlatina, and erysipelas:—

Mr. D.'s family consisted of himself, wife, and two children, one about three and the other about five years old. Whilst I was treating Mrs. D. for diphtheria, in March last, Mr. D. was taken with the same disease. His pulse was frequent and weak; throat badly swollen, with the characteristic ash-colored specks and patches of false membrane; he was very weak and prostrated in a short time after the commencement of the disease. I treated the case with chlorate potassa acidulated with hydrochloric acid, with sul. quinine, tinc. ferri murias, &c. In

about a week the disease subsided, and he began to take nourishment with a relish, when, suddenly, erysipelas commenced on both ears, and spread over the cheeks, but, by pencilling it with nitrate of silver, it was so far arrested that it did not spread over the face and forehead, though it continued over the scalp. Prescribed ℥ss. tinc. ferri murias every four hours, and continued it for four or five days, when the inflammation subsided and he began to recover. Then the right side of his face and neck commenced swelling, and continued to increase until the right tonsil, tongue, and adjacent parts were so much swollen that it was with great difficulty that he could swallow. The inflammation was, in its character, diffuse, with infiltration into the cellular tissue, hard and unyielding, without any signs of pointing, either externally or internally, Under the circumstances, I proposed counsel, and Dr. DAVIS was selected; but, just before he arrived, hemorrhage suddenly commenced from the right ear, when they sent for me immediately, and before I saw him, which was not more than ten or fifteen minutes after the commencement of the hemorrhage, he had lost at least a quart of blood, and was apparently sinking in collapse when Dr. DAVIS arrived. The loss of blood prostrated him very much, and did not in the least diminish the swelling.

My treatment had been tinc. ferri murias with chlorate potassa and quinine as a tonic, and, for a few hours before the arrival of Dr. DAVIS, had been trying to give him in addition sulphite soda, but, owing to the great difficulty of swallowing, did not get down much of anything. Dr. DAVIS recommended a perseverance in the remedies as the only hope. He continued in a collapsed condition for about forty-eight hours and died. We could not obtain the privilege of making a *post mortem* examination, which I very much regret.

How to account for the loss of such a quantity of blood in so short a time I cannot imagine. It was arterial blood, and was poured out in a full stream so that it was caught in a wash-bowl. As before stated, it could not have been more than ten or fifteen minutes after he commenced bleeding before I saw him. I im-

mediately plugged the ear with cotton and arrested the hemorrhage, and it did not return. I know of no artery in the internal ear large enough to discharge such a quantity of blood in so short a time, even if it were severed.

Whilst Mr. D. was sick, both of his children were taken at the same time. The one five years old, with a swelled throat; weak and frequent pulse; with stupor. The other, about three years old, with a scarlet rash, covering every part of the surface, with erysipelas of the inferior extremities. Her pulse was frequent; tongue red; and throat sore, without much swelling. As I was leaving the house the next day, after she was taken, I met Dr. HATCH at the door and requested him to diagnose the case. When he first saw her, he said, it was a well-marked case of scarlatina. After exposing her extremities, he said, it was scarlatina complicated with erysipelas. The treatment of these two cases was the same, viz., one drachm of a saturated solution of sulphite of soda every two hours, continued without intermission for five days, with no other remedy. After taking the sulphite of soda for forty-eight hours, the constitutional symptoms in both cases were entirely relieved, and the swelled throat of the oldest began to subside. The rash and erysipelas of the youngest remained the usual time, and was followed by the ordinary disquamation, though her appetite was good; and the constitutional symptoms subsided the third day, and convalescence was rapid and permanent.

We have here four cases of zymotic disease in one family, at the same time, effecting every member of it differently, viz., one with diphtheria alone; one with diphtheria, followed by erysipelas of the face and head, and diffuse inflammation of the side of the neck; one with scarlatina, with erysipelas of the inferior extremities; and the other, in a stupid condition, with a swelled throat.

Now, it is evident to me that all of these cases originated from the same abnormal condition of the blood; for although there was a material difference in the manifestation, appearance, and location of the disease in the different cases, the constitutional symptoms were very much alike,—all showing a depres-

sion of the vital powers, with other symptoms, such as we have in catalytic or blood-poison diseases, which any physician of experience will surely detect.

Whether the poison which causes these various diseases is the same, generating the same morbid condition of the blood in diphtheria, scarlatina, erysipelas, and other zymotic diseases; or whether there is a specific poison for each of those diseases is a question of importance yet to be solved. The pathological condition of the blood in the so-called blood-poison diseases is attracting the attention of pathologists, both in this country and Europe; and I trust the time is not far distant when we shall understand the nature of such cases better, and be able to treat them in a more scientific manner.

Now, who will not say, that these cases, though differently manifested, originated from the same abnormal condition of the blood? I believe it most sincerely, and that the same constitutional treatment would be applicable to all; and were I to meet with such a group of cases again, I would give the sulphites to all of them, beginning early, and continue them with such other remedies as seemed to be indicated for the local disease.

ARTICLE XXVII.

SINGULAR CASE OF ERYSIPELAS.

By W. H. BYFORD, A.M., M.D., Professor, &c., Chicago Medical College.

The subject of the attack, in this case, was Mr. B., a German, 45 years of age, he is strong and plethoric. He lives in Cincinnati, but was on a visit to his friends in this place. When at home, he occupies a large, well-ventilated house, and lives well, including an habitual, but not intemperate, (in the German sense of the word,) supply of wine and malt liquors. While here, which was but a week or ten days before he was attacked, he occupied a small room with low ceiling in a badly-ventilated house, with two children, his wife, and a servant. In a dietetic point of view, he fared as at home. He is in the habit, for the

last six or eight years, of having a moderate attack of erysipelas once a year. I mention the above facts that they may have due weight in accounting for the singular course pursued by the disease.

He was attacked, June 5th, 1863, with a chill, succeeded by a moderate grade of febrile reaction, which was accompanied with a blush of redness on the middle of the nose. This redness developed into an erysipelatous form of inflammation, sufficiently painful to confine the patient to bed for five or six days. On the 12th, seven days from the beginning, the inflammation had invested the whole head, and the advancing margins met on the occipital region. The skin upon the upper part of the face and forehead had vesicated and was undergoing exfoilation. This process was about completed; the patient had become very comfortable on the 18th, and I took my leave flattering myself that convalescence was almost complete.

Next day, in the afternoon, I was again sent for to see my patient, when I found that a chill and fever, the evening before, had been followed by a recommencement of inflammation on the nose, at the point where the first attack began. The erysipelatous inflammation spread over the same surface, ran its course, and subsided on the 24th. It was not attended with so much distress or fever as the first, but there were febrile reaction, pain, and tumefaction, and the creeping advance of inflammation of erysipelas. When the whole head was surrounded the second time, the inflammation and symptoms subsided, so that, on the 25th, I again took leave of the case, thinking now I was quit of the singular circumstance.

To my astonishment, however, I was called upon to witness a repetition of the same phenomena, which commenced on the morning of the 26th and finished upon the 3d day of July. Thus, my patient had experienced three perfectly developed erysipelatous inflammatory attacks; the inflammation travelling over an extensive surface in a perfect manner each time, in the course of one month, and succeeded each other so closely that they might be considered relapses of the same attack.

The treatment was began, in the first attack, by ordering

four grains of calomel at bedtime, to be followed, in the morning, by citrate of magnesia. Soon as it had operated, the patient was to have twenty drops of the muriated tincture of iron in some water every four hours. He was to have cloths kept constantly wet with apple vinegar on the inflamed part. This treatment was followed steadily for the whole of the first attack; and the tincture was left off only when I ceased my attendance. It was resumed and continued during both of the subsequent attacks, up to the 2d of July. At this time, although there was still some inflammation, I sent him out in the air, and bade him live out door as much as possible, and change his lodgings to a larger and more commodious room. From this time he was convalescent and experienced no further illness, except the annoyance of three or four abscesses under different parts of the scalp, left behind as the effects of the erysipelas. The tincture of iron, acid washes to the inflamed surface, attention to the bowels, and liberal diet constituted the treatment. I have been induced to publish this short sketch of my case from two considerations:—

1st.—In a practice of twenty-five years, during which time I have seen my share of erysipelas, I have, in no instance, known a relapse of erysipelas, that is, an immediate recurrence and prevalence of the inflammation on and over the same surface even once, let alone twice, as in this case; and I have no recollection of reading of an instance of the kind, although I confess not to have examined the subject with special reference to this circumstance; and,

2d.—Because the recurrence took place at a time when the patient might be supposed to be saturated with acid and iron, generally and locally.

I prefer making no comments, leaving my readers to draw their own inferences.

ARTICLE XXVIII.

DENTITION, AND ITS CONNECTION WITH THE BOWEL-AFFECTIONS OF CHILDREN.

By N. S. DAVIS, M.D., Chicago, Ill.

Read to the Chicago Medical Society Sept. 11, 1863.

My object in calling the attention of the Society to a subject so familiar to every member, is to start an influence, if possible, against one of the most prevalent and mischievous popular errors of the day. The error to which I allude consists in attributing to the influence of the first dentition most of the attacks of cholera infantum, diarrhœa, dysentery, etc., that occur in children during every summer. It is well known that the diseases named occur as regular endemics in all the populous towns and cities in this country, during the summer months; that they effect all classes and ages, more or less; but that the two first are much more frequent and severe in children under two years of age than at any other period of life. In this latitude, the attacks of cholera morbus and diarrhœa, in children, usually begin early in July, and continue to increase in frequency throughout that month, although the highest ratio of mortality is in August. Notwithstanding the acknowledged endemic character of these diseases, and the general limitation of their prevalence to the hot months of the year, yet you will find, at least, four cases out of five attributed to "teething." This general assignment of *teething* as the cause of gastric and intestinal derangements, would be of little consequence, were it not for the almost universal neglect of proper treatment in the early stage, which it produces. No incident in practice is more common, during the summer and autumn, than to meet with young children, extremely pale, emaciated almost to the condition of skeletons, and yet having from five to fifteen intestinal evacuations per day, with occasional vomiting. You ask the mother how long the child has been sick? and the answer will generally be: "Oh, its bowels have been out of order all summer; but it has not been, to say, *very sick* until the last week."

You inquire what she has done for the child, and the ready answer will be: "I've done nothing at all, doctor. The child is *teething*, and I thought it would not signify, until the last few days, it has got so weak, I feared it would die." If you push the inquiry further, and ask if she has really let her child go so long with diarrhœa, without giving any medicine or asking any medical advice, she will acknowledge that she has given it castor oil or rhubarb, and sometimes even pink and senna. If you ask, why she thought the growth of the teeth caused the sickness, she will either immediately force the little sufferer's emaciated jaws open and triumphantly point you to the teeth that have appeared, and to the gums which are still enlarged by the advancing teeth; or she will say, that she consulted Dr. A. or Dr. B., and he told her "*it was the teeth*, and that it would not do to stop the diarrhœa *too soon*."

We think no observing physician, in active practice, can have failed to notice the very large number of children, especially among the poorer classes, in whom attacks of summer diarrhœa are entirely neglected until the stage of extreme emaciation and exhaustion has arrived, solely under the idea that such diarrhœa is caused by the growth of the teeth. From direct personal observation, I am satisfied that more than one hundred children, under two years of age, are regularly sacrificed in this city by such neglect during every returning summer and autumn. I mean that more than one hundred children die every season, in this city, from the neglect of the earlier stages of bowel-affections; the excuse for such neglect being that the children were "teething." If this is so, it becomes a very important question, how far the profession is responsible for the existence of the excuse and its consequences? Whether we look into the chapter on the diseases of dentition, contained in most of our works on practical medicine, or listen to the answers given to mothers daily by practitioners, concerning the ailments of children, we shall easily perceive that the chief responsibility in this matter rests directly on our profession. Thirty years since, when I was a student of medicine, it was thought to be as necessary to have a gum-lancet in the pocket as medicine in the medicine-case.

But is it true that the simple natural growth of the teeth frequently produces disease or disturbance of function either in the alimentary canal or elsewhere? To answer this question satisfactorily, we must enquire, first, whether there is anything in the structure of the gum or investing membrane of the tooth, or anything pertaining to their physiology, that would give them extensive sympathetic relations with other parts of the system? second, whether direct clinical observation has shown a frequent coincidence between actual irritation of the gums and bowel-affections? and, third, whether such bowel-affections are generally relieved or cured by any kind of treatment applied to the gums?

In relation to the first question, every anatomist knows that the structure of the gums is composed chiefly of elastic fibrous tissue, with only a moderate supply of capillary bloodvessels, and a still more sparing supply of sensitive nerves. In other words, they belong to the class of dense fibrous structures of low sensibility, and, physiologically, designed to bear pressure and, in a measure, protect the bodies of the teeth in mastication. The only nerves which could give them sympathetic relations with other organs are the branches of the fifth pair, by which they are supplied with sensibility, and the recognized diseases of which certainly do not often cause disturbance of the bowels. Facial and maxillary neuralgias are abundantly common at all periods of human life; and well-known irritation of the investing membrane of the teeth is among the common annoyances of all classes, but I have never yet found a case of the kind productive of sympathetic irritation of the bowels or diarrhœa. Hence, I am compelled to believe that there is nothing in either the anatomy or the physiology of the gums or dental periosteum which could explain the frequent occurrence of diarrhœa in infancy.

Does direct clinical observation show the frequent coincidence of irritation of the gums and diarrhœa in young children? To answer this question, we must first determine what constitutes evidence of irritation in the gum during teething. It will not be denied by anyone that the growth of the tooth and the

gradual enlargement of the gum are strictly physiological processes, and not of themselves evidences of irritation; yet nothing is more common than to find simple enlargement alone presented, not only as evidence of irritation, but as a sufficient reason for scarification; and, of course, as a full and satisfactory cause of whatever derangement or disease may be coexisting at the time; and, inasmuch as there is scarcely any period of time, between the ages of six months and two years, that some of the gums in a child's mouth are not enlarged from advancing teeth, it is easy for either mother or physician to find this condition always present to account for each attack or aggravation of disease that may come to pass. In addition to simple enlargement, the next most frequent evidence of irritation is the disposition of the child to press or bite whatever is put into its mouth. Every practitioner is familiar with the common expression of mothers and nurses, in reference to the causes of illness in their children, "we are sure it is their teeth," say they, "for the gums are not only swelled, but the child is constantly putting its fingers in its mouth, and it bites everything it gets between its teeth." It requires but little observation to satisfy any intelligent physician that this disposition to put things into the mouth and press them with the gums is an instinctive habit of all children in the most perfect health; and, instead of indicating the existence of irritation, affords direct proof that no such condition exists. If any member of this Society has had irritation of his gums or of the periosteum of his teeth, the last thing he was willing to do was to bite, even so softly, on the affected parts. If we ask for the ordinary evidences of local irritation, such as heat, redness and tenderness, we shall rarely find any one of them present in the gums in the bowel-affections of children, except in such cases as are accompanied by fever sufficient to render the whole mouth and all other parts hotter than natural, and such as are accompanied by general inflammation of the mucous membrane of the mouth and fauces. For the purpose of gaining reliable information on this subject, I took the trouble to keep a record of fifty cases of diarrhœa in children under two years of age, during the months of July and August.

The record embraced cases in which the diarrhœa had continued from three days to six weeks, and was of almost every grade of severity. In twenty cases, the gums were *paler* than natural, corresponding with the general palor or anæmic condition of the patients, and entirely free from unnatural heat or tenderness. In twenty-four cases, the color of the gums was natural, without any increased heat or tenderness. In six cases, the gums, together with the whole mucous membrane of the mouth, were more red, more hot, and more dry than natural; and, in three of them, there was apthous ulcerations on the inner surface of the lips and cheeks. In all the six cases, there was general febrile action; and no difference could be perceived between the redness and temperature of the gums enlarged by advancing teeth and those where no such enlargement had taken place. The results afforded by this record are in strict accordance with my observations throughout a professional experience of twenty-seven years; and, if we set aside the simple fact of enlargement of the gums and the disposition of the child to bite on them, I am satisfied that the closest scrutiny will fail to find any other evidences of irritation in them, in ninety out of every one hundred cases of ordinary bowel-affections or sumner complaints of children. But there is another idea in relation to the effect of the growth of the teeth during dentition, which is contrary to the well-known laws of physiological growth. I allude to the idea, that, as the tooth advances, the investing membrane and gum becomes stretched or tense over it, and that such tension is the cause of irritation. It should be remembered that the parts investing a growing tooth are themselves also constantly undergoing change by absorption and nutrition, by which they maintain their relation to the enclosed tooth in every stage of its advancement. During the first fifteen years of my practice, I scarified the gums in several hundred cases; but in no one of them did I ever see the edges of the incision, made over the top of the tooth, recede from each other, as though the tissue was stretched or tense. Indeed, there is no more propriety in supposing the parts investing a tooth are put upon the stretch by its growth, than that the

pericranium and scalp become tense by the growth of the skull.

Does the result of treatment directed to the gums, during the progress of dentition, afford evidence that they are the seat of irritation? In answer to this inquiry, I will simply state the results of my own observations:—When I commenced the practice of medicine, little more than twenty-seven years since, a gum-lancet in the pocket of the practitioner was deemed as necessary as medicine in his saddle-bags; and on almost all occasions of any kind of sickness in young children, if any of the gums were found enlarged simply, it was deemed proper to incise the gums. Consequently, during the first fifteen years of my practice I scarified the gums of children in a large number and variety of cases. During the last ten or twelve years, I have applied the knife to the gums in comparatively very few cases, but have met with many in which the operation had been performed by others. With all these opportunities for observation, I must say, that I have never seen a case of diarrhœa, dysentery, or cholera morbus relieved, in any degree, by any amount of incising or scarifying of the gums alone. Many cases have occurred in which the gums have been cut, and ordinary internal treatment directed at the same time, with speedy relief to the patient; but abundant trials have shown that the same internal treatment, without touching the gums, has been followed by the same results; while not a single instance of the reverse has come under my observation.

It is only a few days since that my attention was called to a child ten months old, pale, anæmic, emaciated almost to a skeleton from diarrhœa, which had continued since the middle of July. Its gums had been scarified four times by a physician in the neighborhood; and he was only deterred from making a fifth trial of the same remedy by the refusal of the mother, on the ground that the previous trials of it had done no good. We thus see that the extremely prevalent idea, that the first dentition is *the cause* of bowel-affections in young children, is not supported either by the anatomy and physiology of the gums and teeth, or by clinical observations at the bedside, or by the results of treatment. Indeed, when we remember that these

affections are limited in their prevalence almost exclusively to the hot months of the year, and to certain geographical limits, we wonder how the idea, that the growth of the teeth acts any prominent part as a cause, could have gained even a respectable degree of credit; for, surely children cut their teeth as often in winter as in summer, in the country as in the city, and as often in one part of the globe as an other.

The remainder of the article relating to the real causes and pathology of the bowel-affections of children, and a glance at the general principles of treatment will be given in the next number of the *Examiner*.

ARTICLE XXIX.

EXPERIMENTS WITH SULPHITE OF LIME AS AN ANTISEPTIC.

By E. ANDREWS, M.D., Professor of Surgery in Chicago Medical College.

M. POLLI, of Milan, Italy, made a large number of experiments upon dogs, tending to show great antiseptic powers in the sulphites. Analogous to this quality is their well-known power of arresting fermentation in cider and other liquors. M. POLLI, in one of his experiments, took two dogs, to one of whom he gave a quantity of the sulphites and to the other none. After some hours, both were killed and laid aside. He reports that the carcase of the dog which took nothing readily putrified, while that of the one which took the sulphite remained a long time perfectly fresh. During the hot weather of August, it occurred to me to try the experiment of the direct application of the sulphites to animal matters, thinking that if successful in preserving them from putrifaction, they would be an excellent application to suppurating wounds, and would prevent the formation of the septic poison of erysipelas.

I accordingly took four ounces of fresh beef, and cut it into two equal parts. Each fragment was placed in a wide vial, containing water enough to cover the beef. Into one of them

I dropped a drachm of sulphite of lime and shook up the bottle. The other contained only the beef and water. The two were placed on a shelf and left open. At the end of forty-eight hours, both were putrid and very offensive; and, so far as I could determine, there was no ground of choice between the odors of the two vials. This would seem to prove that sulphite of lime has no power of preserving animal substances, when applied to them directly. It, probably, requires the presence of an acid to combine with the lime and liberate free sulphurous acid to make it active. The acids of the stomach might serve this purpose, when given internally.

My object in selecting the sulphite of lime in preference to that of soda was, because the former, being insoluble, could be applied to a wound or ulcer without producing smarting. Notwithstanding the failure of the meat experiment, I tried the article externally in two cases where I had operated for the removal of necrosed bone. I mixed the sulphite of lime with clear water, and injected it freely into the wounds several times a-day for about ten days. The cases both did extremely well; but, as I took great care of them in every other respect, I am not able to say whether the local use of the sulphite was entitled to any credit.

Selections.

LECTURE ON ECZEMA,

INCLUDING ITS IMPETIGINOUS, LICHENOUS, AND PRURIGINOUS VARIETIES,

DELIVERED AT THE

DISPENSARY FOR SKIN DISEASES, GLASGOW, SCOTLAND.

By T. McCALL ANDERSON, M.D., F.F.P.S., Physician to the Dispensary for Skin Diseases; Physician to the Deaf and Dumb Institution, etc.

Reprinted from the "Medical Times and Gazette."

Gentlemen:—From the treatment of eczematous eruptions occurring in limited patches, I pass naturally to the consideration of the last division of the subject, the

Local Varieties of Eczema, and I trust you will bear in mind

that the remarks I am about to make are to be taken in connexion with what I have already said, as much needless repetition may thus be avoided.

While eczema may be observed upon any part of the cutaneous envelope, and indeed may affect almost the whole of it at one time, there are certain localities which it seizes upon in preference to others, and to which it is often limited. These are the head, hairy portions of the face, lips, edges of the eyelids, nostrils, external auditory passages and ears, hands, feet, legs, genitals, anus, umbilicus, and those parts of the skin which are naturally in contact with one another.

Eczema of the Head (eczema capitis, impetigo capitis) occurs most frequently in the pustular form, especially in the case of children, whose heads are attacked with remarkable frequency. When this part is affected, the eruption has a tendency to chronicity, particularly if the treatment is not energetically and thoroughly carried into effect. For it is more difficult to keep the surface clean than when the non-hairy parts are invaded, owing to the hairs being glued together by the exudation, and to the crusts being entangled in them and difficult of removal. For this reason, the patient often allows them to remain for weeks, months, nay, even years upon the head, and, when advice is at last obtained, the whole scalp is not unfrequently found to be concealed from view. In this way, collections of pus are often formed between the crusts and the scalp, owing to confinement of successive exudations, which do infinite harm. Besides, when hard crusts are allowed to remain on the head for a lengthened period of time, they press upon the hair follicles, and lead to their obliteration, whereas, when the eruption is properly treated from the first, there should be no permanent loss of hair. When the disease is neglected in the manner just indicated, the exudation becomes decomposed, lice are attracted to the part, and are often detected, wallowing in the mire, in thousands, while their nits (eggs) adhere, by means of sheaths, with great tenacity to the hairs, and in countless numbers. But while lice often occur as complications of an eczematous eruption, you must be alive to the fact, that these insects sometimes attack the head of a healthy person, in whom they excite a sensation of itching. This causes the patient to scratch the head, and an eczematous eruption may thereby be induced. The lice on the head are thus the exciting cause of eczema in some cases, its result in others.

Little sub-cutaneous abscesses are often met with on the head in addition to the collections of matter between the scalp and

the crusts, and enlargement of the neighboring glands, especially those on the back of the neck and over the mastoid processes, occur in all aggravated chronic cases. These enlargements are due to the irritation set up by the adjacent eruption, and must on no account be looked upon as indicative of a strumous taint, though, of course, a smaller amount of irritation is capable of causing their enlargement in scrofulous children.

The *diagnosis* of eczema capitis is sometimes difficult to the unaccustomed eye, and I have accordingly arranged in a tabular form the points to be attended to as distinguishing it from syphilitic eczema capitis, seborrhœa capitis, psoriasis capitis, and herpes tonsurans.

Table showing the Points which distinguish Eczema Capitis from Syphilitic Eczema Capitis, Seborrhœa Capitis, Psoriasis Capitis, and Herpes Tonsurans:—

Eczema Capitis.	Syphilitic Eczema Capitis.
(1.) Occurs oftenest in children.	(1.) Occurs usually in adults.
(2.) Often attacks the whole scalp.	(2.) Usually occurs in small patches.
(3.) Exhibits superficial ulcers only.	(3.) Exhibits deep ulcers, with perpendicular edges and unhealthy bases.
(4.) Occurs in persons in whom there is no history of primary syphilis, except as a coincidence.	(4.) Occurs in persons in whom there is a history of primary syphilis.
(5.) Does not occur in connexion with symptoms of syphilis, except as a coincidence.	(5.) Occurs in connexion with other signs of syphilis, *e.g.*, alopecia, sore throat, other syphilitic eruptions on the skin, glandular enlargements, rheumatic pains, etc.

For further particulars, see the general diagnosis of eczema from syphilitic eruptions.

Eczema Capitis.	Seborrhœa Capitis.
(1.) Exhibits crusts, which are brittle, are often very thick, and are composed of pus, granular matter, and epithelium.	(1.) Exhibits crusts, which can be kneaded into a ball, are usually thin, have an oily feel, and are composed principally of sebaceous matter and epithelium.

(2.) Is excessively itchy, and after removing the crusts, the scalp is infiltrated, red, often excoriated, and exudes serum or pus.

ECZEMA CAPITIS.

(1.) Occurs oftenest in those whose health is deteriorated, or who are scrofulous.

(2.) Is usually very itchy.

(3.) Exhibits exudation on the surface of the skin.

(4.) Exhibits thick yellowish, usually moist crusts.

(5.) Occurs often in connexion with eczema of other parts, as of the ears, etc.

(2.) Is not excessively itchy, and, after removing the crusts, the scalp is not infiltrated, red, or excoriated, exudes neither serum or pus, but is smooth and oily.

PSORIASIS CAPITIS.

(1.) Occurs oftenest in those who are apparently in perfect health, and rarely, if ever, in those who are scrofulous.

(2.) Is usually not very itchy.

(3.) Is a perfectly dry eruption.

(4.) Exhibits usually thin, white, dry scales.

(5.) Occurs almost invariably in connexion with psoriasis of other parts, especially of elbows and knees, where the diagnois is easy.

ECZEMA CAPITIS.

(1.) Patches not circular unless the hair has been cut short in a circular manner with scissors.

(2.) Hairs healthy, (though they may fall out here and there,) and exhibit no parasite.

(3.) Itching usually excessive.

(4.) Eczematous eruptions often on no other part of the body.

(5.) Not contagious.

HERPES TONSURANS (Ringworm of the Head).

(1.) Patches circular.

(2.) Hairs brittle, twisted, broken off close to the scalp, loaded with the parasite (tricophyton tonsurans).

(3.) Itching not usually severe.

(4.) Herpes circinatus (ringworm of the body) often on the body.

(5.) Contagious, especially to children, and often other members of the family exhibit ringworm of the head or body.

But cases are often met with in which ringworn of the head is complicated with eczema of the head. The eczematous eruption is then the more prominent of the two, and the ring-

worm is often overlooked. In these cases, the diagnosis is arrived at by detecting some of the brittle, broken, or twisted hairs loaded with the parasite. It is, therefore, well, in every case of eczema, to examine the hairs carefully, with the eye at least. The history of the case, the way the eruption commenced, (in circular dry patches,) and the signs of the contagious nature of ringworm, assist the diagnosis. The following case is a good example of the complication of ringworm of the head with eczema:—

Richard R., aged 8, was admitted at the Dispensary for Skin Diseases, November 25, 1861. Almost the whole of his head was covered with a thick, yellowish, eczematous crusts, and the backs of his ears were infiltrated, exuding, and itching. Little patches of alopecia existed on the scalp, and on examining the head attentively, little fragments of hairs were detected here and there, which were brittle, broke on attempting to extract them entire, and were loaded with the spores of the tricophyton tonsurans. The disease commenced as a small circular patch on the crown of the head, having, according to the statements of the mother of the patient, all the characters of ringworm.

Mr. Jabez Hogg, who professes to find parasitic growths in all skin diseases, would, probably, have made use of this case in support of his views, had it come under his observation, whereas a careful examination, and an inquiry in the history of the case, showed it to be a case of ringworm, complicated, in its later stages, with eczema.

Alopecia areata (circular patches) of baldness ought never to be mistaken for eczema of the head, and the disease only requires to be kept in view in order to prevent an error in diagnosis. *Favus* is, however, often difficult of distinction from eczema capitis. In cases of favus, "where the head is more or less covered with an eruption exhaling the odor of mice, and consisting of bright yellow dry crusts, depressed in the centre, through the middle of each of which one or more hairs pass, which have a dull, dry appearance, and are more easily extracted than natural, the diagnosis is very easy, and those who have seen the disease once can never mistake it. When it has continued for a length of time, when the crusts have lost their cup-shaped form and their bright yellow color, and have become entangled in the hair; when, in fact, we have to do with the variety described as favus squarrosa, it may be, —and often is—mistaken for impetigo of the scalp. But in the former there are generally patches of alopecia which are

wanting in the latter. In it certainly the hairs often fall out, although only here and there, and not in patches as in favus. The alopecia of favus is permanent, that of impetigo generally temporary. There is also no alteration of the hairs in the latter; in the former they are dull, dry, discolored, and easily extracted. Attention to these points generally serves to clear up the diagnosis; but, if doubt still exists, it may at once be removed by the microscopic examination of the crusts. There is one point, however, which requires to be borne in mind, namely, that the discovery of some pustules does not prove that the disease is impetigo, as pustules are frequently developed in cases of favus from the irritation of the parasite. And also one should not lay too great stress on the value, in a diagnostic point of view, of the odor exhaled from the eruption, as this symptom is not so pathognomic as some dermatologists would have us to suppose.

"Very often the diagnosis is rendered difficult on account of a propensity of parents to clean carefully, and remove all the crusts from the head before bringing their children for advice. There is then to be seen redness of the scalp combined with the presence of a few pustules, the results of irritation, and here again the disease resembles impetigo. But if it is a case of favus which we have before us, the deep red, depressed, distinctly circumscribed surface, covered by a thin, shining epidermis, is quite different from the light-colored, diffused redness of impetigo. If this is not sufficient, the hairs should be examined, when they will be found to be altered, and the parasite is detected in them with the microscope. If this is not satisfactory, do not give an opinion or resort to any treatment, but desire the patient to return in a couple of weeks, leaving the head untouched in the *interim*, after which time the disease will have had time to re-develope itself, and its nature is at once discovered.*

In the *treatment* of eczema of the head, the removal of the crusts is often difficult, and the remedial applications cannot be so thoroughly applied to the diseased surface. If the eruption is at all extensive or severe, and if it occurs in children, I am in the habit of ordering the hair to be cut as short as possible, and I always insist upon this, if, as happens too often, particularly amongst the poor, the disease is complicated with lice. The crusts can then be separated with greater facility, and the morbid surface more fully exposed to view. In adults,

* "The Parasitic Affections of the Skin," by T. McCall Anderson, M.D. London: Churchill & Sons. 1861.

and especially in females, the removal of the hair must often be dispensed with, and the cure is, consequently much retarded.

It is only in chronic cases, occurring in adults, and rebellious to milder treatment, that shaving of the head and the application of iodine and blisters (see Lecture in Sept. No.,) is to be recommended; for, in the ordinary run of cases, the milder treatment, before fully discussed, is usually effectual. In very obstinate chronic cases, which resist both internal and external remedies, although very few indeed do not yield to blisters, epilation may be tried, though this is rarely necessary.† If epilation is recommended, and you must remember that it is only to be carried into effect as a "*dernier resort*," you should at the same time use a lotion of bichloride of mercury, or one of the mercurial ointments previously referred to. (See Lecture in Sept. No.)

Eczema of the Hairy Portions of the Face (Eczema pilare faciei), is an exceedingly common, and a very annoying affection, owing to the disfigurement which it occasions, the burning heat which accompanies it, and the difficulty and pain of shaving. The only word in English dermatological works which is intended to denote it, is "impetigo menti," but the disease is by no means confined to the chin, so that this name is a too restricted one. I have, therefore, called it "Eczema pilare faciei," which is more correct, though, perhaps, not so euphonious.

The eruption commences by the formation of pustules, each of which is situated at the orifice of a hair follicle; for you will notice that a hair passes through the centre of each pustule. It is curious, though true, that eczema almost always assumes the pustular form in this situation in adult males,—an observation which coincides with what I told you before, that the pustular form of eczema (impetigo) is much more frequently observed on hairy parts of the body.

These pustules dry up into small yellow crusts, which are difficult of removal, owing to their adhesion to the hairs as well as to the skin. When many pustules form at the orifices of neighboring follicles, they have a tendency to run together, and, on drying up, large irregular yellow crusts are left. If these are not removed, successive exudations on the surface of the skin are confined by them, and lead to excoriations, and, owing to their continued pressure, to obliteration of the hair follicles, and permanent alopecia of the affected parts. The

† For the mode of extracting the hairs, see my work on the "Parasitic Affections of the Skin," p. 34. London: Churchill & Sons. 1861.

skin on which the pustules are developed assumes a dusky red tint, and becomes gradually more and more thickened and infiltrated. The patient sometimes complains of itching, oftener of pain or burning heat,—a sensation which is principally experienced during the formation of a crop of pustules. The disease is often kept up for months or even years, owing to the occurrence of successive crops of pustules.

The causes which specially operate in the production of this form of eczema are, irritating discharges from the nose and mouth, and the irritation of the razor, especially when that instrument is blunt. Indeed, the disease not unfrequently disappears spontaneously when the causes are no longer in operation, unless the predisposition to eczema is very strong, or the eruption has lasted a long time.

The diseases with which it may be confounded are acne sycosiformis and sycosis parisitica. In *acne sycosiformis* the eruption partakes more of the tubercular than of the pustular form, and the eruption of acne is detected on the non-hairy parts of the face, and often also between the shoulders and on the front of the chest. Indeed, acne sycosiformis (so named from the resemblance of the eruption to sycosis parasitica,) is merely acne indurata implicating the hairy instead of the non-hairy parts of the face. Strictly speaking, the form of eczema at present under consideration has nearly as much right to be ranked with acne as with eczema, in which case, acne sycosiformis would be regarded as an advanced stage of eczema of the hairy parts of the face. The treatment of the two affections is nearly the same, so that it unnecessary to insist further upon the diagnosis.

The disease which is oftenest confounded with this form of eczema is sycosis parasitica, although the differences are generally very marked. These are well shown in the following table:

Eczema Pilaris Faciei.	Sycosis Parasitica (Ringworm of the Beard).
(1.) Very common in this country.	(1.) Very rare in this country.
(2.) A pustular disease only.	(2.) Pustules, tubercles, and large fleshy indurations detected when disease fully established.
(3.) No trace of herpes circinatus either on the affected parts or in other localities.	(3.) Rings of herpes circinatus (ringworm of body) detected amongst the hairs,

(4.) Not contagious.

(5.) Hairs healthy and adhere firmly, so that epilation causes pain, unless much suppuration has occurred at their roots.

(6.) No parasite to be detected.

and often round the front of the neck, or on the wrists, arms, or other parts of the body.

(4.) Contagious, and often history of contagion.

(5.) Hairs brittle, broken, and twisted; have lost their natural glistening appearance, and can be extracted with perfect ease and without pain.

(6.) Fungus (tricophyton tonsurans) detected in the hairs and scales.

In the cure of this form of eczema, if the means already indicated fail, the patient should be directed to stop shaving, or, if he has a beard, the hair should be cut short. All the hairs proceeding from the affected parts should then be extracted, after which a stimulating and alterative ointment, such as citrine ointment, should be rubbed firmly over the morbid surface night and morning. This treatment often acts like a charm, and I have cured many old standing cases in a couple of weeks by means of it. While it sometimes fails to effect a complete cure, it is always, as far as my experience goes, productive of temporary benefit. After the parts have been once epilated, if new pustules appear, the hair passing through the middle of each must at once be extracted, and the use of the ointment continued.

The following case, and I could cite many such, illustrates the benefit of this mode of treatment. I have selected it because it shows the value of epilation as compared with other treatment:—

Mr. M., aged about 35, consulted me on April 24, 1861, with regard to an eruption on the upper lip, immediately beneath the nostrils. The patch was about an inch square, the skin red and infiltrated, and numerous pustules and yellow crusts were situated at the orifices of the hair follicles. The disease was kept up by the formation of successive crops of pustules. He stated that he had frequently discharge from the nostrils, which he thought irritated the skin of the upper lip. He had been taking Donovan's solution for some time when I saw him, and he said with benefit, and it was, therefore, continued.

A week afterwards, the eruption being in no way altered, Fowler's solution, at first in ten, later in fifteen-drop doses

(thrice daily), was administered for some weeks, and an ointment of two drachms of citrine ointment, mixed with six of linimentum calcis, was rubbed firmly into the roots of the hairs night and morning. The arsenic, in one form or another, having been continued for a couple of months, and pushed till it produced derangement of the digestion, and no benefit accruing from its employment, was omitted, and the morbid surface was touched gently with solid potassa fusa after the removal of the crusts.

A week afterwards, (May 21, 1861,) great improvement was observed. The infiltration and redness of the skin were much less, but still a few pustules continued to form at the edges of the patch.

I now lost sight of the patient till January 23, 1862, when I found the eruption pretty much in the same state as when I first saw him, it having never disappeared entirely. I at once removed the crusts, extracted all the hairs, and ordered citrine ointment to be used night and morning.

Four days later, (January 27, 1862,) the infiltration and redness of the skin were nearly gone, and no new pustules had appeared. He was ordered to continue the use of the ointment a little longer, and, if any new pustules appeared, to pull out the hairs which proceeded through the centres of them.

About two months after this, (March 21,) I saw this gentleman by accident, when he informed me that, since the epilation, the disease had never reappeared, and I could discover no trace of the previous eruption. He wore a magnificent moustache,—epilation, as most of you are aware, having the effect of making the hair grow more luxuriantly than ever, owing to the stimulus which that operation gives to the circulation of the part.

Those of you who are alive to the benefits of a luxurious pair of whiskers, and who have not yet succeeded in the attainment of your wishes, may, perhaps, be inclined to draw a practical lesson from the results of epilation in the case of the gentleman just alluded to.

Eczema of the Lips (eczema labiorum) is by no means of rare occurrence, and may coincide with a similar eruption on other parts, though the former are often affected alone. The eruption may be confined to one lip, or both may be implicated, and they may be the seat of any of the forms of eczema previously described. They are often greatly swelled, the serum being diffused through the cellular tissue, the meshes of which are very loose. The oral aperture is often spasmodically

contracted, especially if fissures complicate the eruption, as they often do, particularly at the angles of the mouth and the centre of the lower lip.

Hebra has observed eczema of the lips to be frequently associated with eczema of the anus, and he once had a patient who was affected alternately with eczema of the anus and lips.

The two diseases which are most apt to be mistaken for eczema of the lips are, herpes labialis and syphilitic eruptions on these parts. But you will be little likely to fall into error if you remember the points to which I referred in speaking of the diagnosis of eczema in general. There is just one additional circumstance, however, with which you should be familiar in connexion with syphilitic affections of the lips, namely, that the eruption rarely affects the whole of even one lip, but has a marked tendency to concentrate itself at the angles of the mouth, where it is often obstinate till the patient is brought under the influence of mercury, when it, "vanquished, quits the field."

You must be careful in the use of strong solutions of poisonous preparations, such as those of corrosive sublimate, in the treatment of this affection, for it is quite possible for the patient to swallow a sufficiency of the mixture to induce serious symptoms. I have nothing to add with regard to treatment further than to refer you particularly to my remarks upon the means adopted for the removal of limited eczematous eruptions.

The following case of eczema of the lips is a good illustration of the eruption in question:—

"A gentleman, aged about 35, consulted me on April 15, 1861, about an eruption of eczema attacking both lips, and for the second time. A small infiltrated exuding and itching patch existed on the right cheek, near the angle of the mouth, and occasionally vesicles were detected on it. The lips were slightly infiltrated, thickened, red, and itching; the epithelium was constantly peeling off them, so that they were very rough, and sometimes a little serous fluid exuded, while fissures had formed here and there, but particularly at the angles of the mouth. His general health was excellent. Fowler's and Donovan's solutions were successively administered without effect, and the disease was finally and rapidly cured by applying "aqua potassa" to the parts night and morning, and washing them frequently with cold water."

Eczema of the edges of the Eyelids (*Eczema tarsi*, *Ophthalmia tarsi*, *Tinea ciliorum*,) is exceedingly common, especially in scrofulous children, and in them often associated with con-

junctivitis and strumous ophthalmia. The affection is neither more nor less than a pustular eczema (impetigo), attacking the edges of the lids, although is does not seem to be generally recognized as such by ophthalmic surgeons; for it commences by the formation of pustules at the orifices of the hair-follicles, which concrete into scabs, beneath which small ulcers are detected, and, when the disease is fully developed, the usual symptoms of eczema, itching, infiltration, exudation, etc., are observed. The exudation from the morbid surface, mingled with the altered secretion from the Meibomian follicles, glues the edges of the eyelids together, especially at night, unless proper precautions are taken. Lachrymation is likewise a common symptom, and the tears, falling on the cheek, not unfrequently irritate the skin, and give rise to an eczematous eruption.

If improperly treated or neglected, as occurs too often, the pressure of the crusts, the confinement of the discharge, and the formation and extension of ulcers, lead ultimately to obliteration of the Meibomian glands and hair-follicles, after which, a perfect cure is of course impossible. Amongst the train of evils may also be mentioned eversion or inversion of the lids, and, if the eyelashes are not gone, owing to obliteration of their follicles, the hairs are apt to assume abnormal directions.

With regard to the local treatment, (for the constitutional, see the treatment of eczema generally), the extraction of the eyelashes is always followed by improvement. This operation is far too often omitted, for, in my opinion, it should be uniformly carried into effect in bad cases, and repeated if new pustules form at the orifices of the follicles, exactly in the same way as in the treatment of eczema of hairy parts of the face. (See treatment of eczema of hairy portions of face.) In addition to this, if the parts are much infiltrated, I am in the habit, after the removal of all crusts, of applying a solution of potassa fusa (usually a solution of ten grains in an ounce of water,) to the edges of the lids, an operation which should not be entrusted to the patient, at first at all events. A small brush must be used, and very little of the solution taken up by it, so as to make it moist, but no more. The eyelid must then be carefully dried, else the application spreads, everted so as to remove it from the eyeball, and the solution painted along its edge. A large brush soaked in cold water should be in readiness to stop the action when desired. This application may be repeated every day till the infiltration, exudation, and itching subside, after which citrine ointment may be relied upon for completing

the cure. In slight cases, you will often find the eruption yield to the use of citrine ointment alone, coupled with cleanliness. During the treatment of eczema tarsi, a little ointment (diluted citrine ointment, for example,) should be applied to the edges of the lids at night, so as to prevent their adhesion, but it must always be washed off in the morning. If, notwithstanding the anointing of the lids at night, they are adherent in the morning, you must on no account tear them asunder, but must previously soften the agglutinated matter. Towards this end you cannot go wrong if you follow the instructions of Mackenzie:—"A teaspoonful of milk, with a bit of fresh butter melted in it, may be employed for smearing the lids, rubbing it with the finger gently along the agglutinated eyelashes. A piece of soft sponge, wrung out of hot water, is then to be held upon the eyelids for some minutes, after which the patient will find the eyelids yield without pain to the least effort he makes to open them. With the finger-nail the whole of the matter is immediately to be removed."*

If there is any inflammation of the conjunctiva, Mackenzie's excellent wash of the bichloride of mercury may be used with advantage, and is often sufficient when the conjunctivitis is slight.

℞. Hydrargyri Bichloridi,..... gr. j.
Hydrochloratis Ammoniæ,..... gr. vj.
Cocci Cacti,..... gr. jss.
Alcoholis,..... ℥j.
Tere simul, adde aquæ uncias sex, et cola per chartam.

Siqua.—Pour out half a tablespoonful of this fluid, and mix it with as much boiling water in a teacup previously warmed. With a piece of old linen or soft sponge bathe the eyelids with the mixture for a few minutes, and then, by leaning back the head, allow a little of it to flow in upon the eye. Repeat this thrice daily.

For the treatment of a more severe attack, as well as for that of the other complications of eczema tarsi, such as ectropium, entropium, trichiasis, ophthalmia scrofulosa, etc., I must refer you to special works on ophthalmic surgery.

* "A Practical Treatise on the Diseases of the Eye," by W. Mackenzie, M.D. Fourth Edition, p. 145. London: Longman, Brown, Green, and Longmans.

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LECTURE ON CERTAIN DISORDERS OF THE BRAIN AND NERVOUS SYSTEM,

WITH SPECIAL REFERENCE TO THE CHANGES IN OPINION AND PRACTICE WHICH RESULT FROM RECENT RESEARCHES IN PHYSIOLOGY & PATHOLOGY.

DELIVERED AT THE

ROYAL COLLEGE OF PHYSICIANS.

By CHARLES BLAND RADCLIFF, M.D., Fellow of the Royal College of Physicians, Physician to Westminster Hospital, etc.

LECTURE V.—PART II.

(3) ON THE PATHOLOGY OF CONVULSION AS DEDUCED FROM THE CONDITION OF THE INNERVATION IN THIS DISORDER.

Reprinted from the "London Lancet."

86. *The signs of a weak or jaded brain are scarcely ever absent in persons who are liable to epileptic and other chronic forms of convulsive disorders.*

It is an indisputable fact, that idiots are very often epileptic, and it is surely no mere accident that the head of the epileptic is so often wanting in proper dimensions and proportions as to suggest to the least imaginative observer its near kinship to the head of the idiot. It is also an indisputable fact, that a not uncommon ending of epilepsy is in dementia, and that there are very few, if any, cases of epilepsy in which the shadows of this dementia are not forecast upon one or other of the mental faculties, especially upon the memory. Nor is it an objection to this view that men like Julius Cæsar, or Mahomet, or Napoleon I., should have had epileptic seizures; for who shall say, that the overwrought brains of these men may not have been broken down in some important particulars before these seizures made their appearance? And, certainly, there is no lack of evidence to show that the subjects of hysterical or chronic convulsion are in all respects the very reverse of "strong-minded."

87. *All signs of mental life are abolished, or on the point of being abolished, during the paroxysm of convulsion.*

In general, convulsion of an epileptic or epilepti-form character the mind is a perfect blank, and so also, with very few exceptions, in the partial forms of the same convulsions. In the convulsion of hysteria, and in the more severe forms of choreic movements, and the will is altogether in abeyance, and the intellectual state is one which approaches very closely to that of unconsciousness. Hence it is not to be supposed that

convulsion is in any way connected with exalted functional activity of that part of the brain which ministers to the mental faculties.

88. *Convulsion must not be looked upon as a symptom of active inflammation of the brain or its membranes.*

In acute inflammation of the brain or its membranes, convulsion may occur during the cold stage of the disorder, or, more rarely, when the stage of true inflammation is subsiding into the stage of coma or collapse, but never during the existence of active phrenitis. It seems, indeed, to be a constant rule that the hot stage of active inflammation of the brain or its membranes is attended, not by convulsion, but by acute delirium, such as is met with in phrenitis. In the more passive forms of this inflammation, convulsion is a much more frequent phenomenon than in the acute forms, and it is not so easy to determine the relations of the convulsion to the inflammation; but, so far as I can make out, convulsion is altogether incompatible with the presence of the periods of vascular reaction, even though these periods be but faintly marked. In the great majority of these cases, indeed, true inflammatory excitement of the circulation is always absent; and the delirium which may be present is no proof to the contrary, for this delirium is of the type which is met with in delirium tremens, and not of the type which is met with in acute phrenitis. It would seem as if the cerebral disorder in these cases did not get beyond the stage of rigor,—the stage of "irritation," of which I shall have more to say presently. At any rate, I know of nothing in the clinical history of these passive forms of inflammation of the brain or its membranes which is at all calculated to contradict the previous conclusion, that convulsion is altogether incompatible with the period or periods of vascular reaction. Nor is there any evidence of a contrary character in the disclosures of the dead-house.

89. *Convulsion must not be looked upon as a symptom of a congested condition of the cerebral veins.*

As it seems to me, the clinical history of disease is opposed to the theories which ascribe convulsion to a congested condition of the cerebral veins. In whooping-cough, these veins are often congested in a very high degree during the paroxysm, and yet convulsion is only an accidental accompaniment of the paroxysm. In congestion of the lungs, also, these veins are greatly gorged with black blood; and the consequence of this engorgement are dreamy sleepiness, stupor, perhaps, coma, rarely convulsion. Nor is the case different where extreme

venous congestion of the brain is brought about by straining or in any other way; for here the symptoms are coma and paralysis, not coma and convulsion; apoplexy, not epilepsy. Moreover, the recent experiments of MM. Kussmaul and Tenner show very plainly that the effect of tying the internal and external jugulars of rabbits is *not* to convulse these animals, but only to stupify them for the first twenty-four or thirty hours, and, in some instances, to cause them to gnash their teeth for a short time. Indeed, there is nothing in all the evidence, physiological or clinical, to nullify the conclusion already drawn,—that venous blood has no special action in producing convulsion.

90. *The peculiar condition of the nervous system which is known under the name of "irritation," and which, in the majority of cases, has a great deal to do with the production of convulsion, is in no sense the equivalent of inflammation.*

The morbid condition of the nervous system, which is known under the name of "irritation" may be attended by various definite symptoms, and followed by various definite results. According as it may happen to affect the parts of the nervous system which minister to ordinary muscular movements, to common or special sensation, or to vascular movements, it may be attended by tremor, convulsion, or spasm, by morbid sensations of various kinds, or by a contracted condition of the vessels. According as it may happen to have affected one or other of the parts of the nervous system which have been named, it may be followed by paralysis of the ordinary muscular movements, by anæsthesia or some analogous condition of special sensation, or by congestion and inflammation. Moreover, it may be attended by partial or general delirium, and followed by partial or general stupor or coma, if its seat happen to be in the parts of the nervous system which minister to volition and reason. Nor is it difficult to see how it should be thus attended and thus followed.

The circumstances which give rise to the state of "irritation" in some part of the nervous system, and which favor the development of this state, are circumstances which appear to produce the reversal of the natural electrical relations of the exterior and interior of the nerve-fibres at the seat of "irritation." It has been seen in a former lecture that the natural electrical relations of the exterior and interior of the nerve-fibres are reversed when these fibres are acted upon by certain mechanical injuries or when their vitality is upon the point of extinction. It has been seen, that is to say, that the natural

electrical relations of the exterior and interior of the nerve may be reversed under circumstances which may give rise to the state of "irritation" in a nerve or nerve-centre, and which favor the development of this state: for nothing is more certain than that mechanical injuries to a nerve or nerve-centre may give rise to a state of "irritation" in these parts, and that a depressed condition of the vital powers generally is favorable to the development of this morbid state. And thus there is no difficulty in supposing that the natural electrical relations of the exterior and interior of the nerve-fibres may be reversed at the seat of "irritation."

Now, in the case where the natural electrical relation of the exterior and interior of the nerve-fibres are preserved, all parts of the exterior of the fibres are electrified positively, and all parts of the interior negatively, and the electric condition of the exterior and interior appears to be one of static tension, so long as the nerve-fibre remains in a state of inaction. And this appears to be the case, because *all* parts of the exterior are electrified positively, and because *all* parts of the interior are electrified negatively; for it is a law of electricity that parts electrified with *similar* electricity repel each other. Nor is this condition of static tension *in* the exterior and *in* the interior of the nerve-fibres neutralized by a contrary action *between* the exterior and interior; for there is something in the constitution of the fibres, be that what it may, which keeps the exterior and interior in opposite electric conditions, and which prevents the opposite electricities of the exterior and interior from rushing together and neutralizing each other. But in the case where the natural electrical relations of the exterior and interior are reversed in a certain part of a nerve-fibre, instead of all parts of the exterior being electrified positively, and all parts of the interior negatively, some part of the exterior is electrified negatively, and some part of the interior positively, as is shown in the accompanying diagrams by making the parts of the fibre, which are occupied by negative electricity, of a darker shade than the parts which are occupied by positive electricity: and the result is that there must be a continual combination and disappearance of electricity *in* the exterior and *in* the interior between the parts of the fibre in which the natural electrical relations of the exterior and interior are preserved, and the part in which these relations are reversed,—between, that is, the parts which are indicated in the diagram by the letters AA, and the part which is indicated by the letter B. The result, that is to say, is one in which the nerve must

lose electricity so long as this reversal continues; for it is a law of electricity that opposite electricities combine and neutralize each other if there be nothing to prevent them from yielding to their natural affinities. In other words, the result of the reversal must be to throw the nerve from the state of inaction into that of action; for it has been seen that a nerve loses electricity when it passes from a state of inaction into that of action, and that this loss is accompanied by the development of instantaneous electrical currents of high tension, analogous to the discharges of the torpedo in and near the nerve.

If, then, this be so,—if the natural electrical relations of the exterior and interior of the nerve-fibres are reversed at the seat of "irritation," and if the effect of this reversal is to throw the nerve from a state of inaction into that of action so long as the reversal continues,—then there is no difficulty in explaining the several phenomena which may attend upon or result from the morbid state of the nervous system which is known as "irritation." There is no difficulty in understanding how, according as its seat may be in ordinary motor nerves or ganglia, in sensory nerves or ganglia, in the cerebral hemispheres, or in the vaso-motor nerves or ganglia, the "irritation" may be attended by morbid contractions of the common muscles, by morbid sensations of various kinds, by morbid mental movements, or by morbid contractions of the bloodvessels; and how, according as its seat may have been in one or other of those parts of the nervous system which have just been mentioned, the "irritation" may end by paralyzing one or other of these parts, and by producing in this manner paralysis of the ordinary muscles, or anæsthesia and conditions allied to it, or partial and general stupor and coma, or congestion and inflammation.

According to this view, then, it is easy to see how, if the vaso-motor nerves or ganglia are implicated, the "irritation" may be attended by contraction of the vessels; for contraction of the vessels is produced when these nerves are acted upon by galvanism or in any other way. And it is as easy to see how, in the end, the same "irritation" may be followed by congestion and inflammation: for it has been seen that the contraction of the vessels passes into congestion and inflammation if the said nerves or ganglia be paralyzed either by the continuance of the same cause which first put them in action, or in any other way. (Indeed, the experiments of Professor Claude-Bernard and Dr. Brown-Séquard upon the sympathetic in the neck appear to furnish the key to the interpretation of inflammation when it is used in this manner.) Nay, it is easy to see how this conges-

tion or inflammation may be seated anywhere or everywhere; for there is no part of the nervous system in which the "irritation" may not begin, or into which it may not travel, and therefore there is no part of the vaso-motor system which may not be paralyzed by the continuance of the "irritation,"—paralyzed, that is, by the continuance of the action of those instantaneous electrical currents of high tension, analogous to the discharges of the torpedo, which are developed in and near the nerve or nerve-centre, which is the seat of "irritation."

According to this view, then, "irritation" is in no sense the equivalent of inflammation. According to this view, indeed, the *hot stage* of inflammation, with its congested and inflamed vessels, *must follow the cold stage*, with its rigors and pains, and with its shrunken and contracted vessels, because the hot stage is not produced until the "irritation," which produces at one and the same time and by one and the same means the rigors and pains and empty vessels of the cold stage, has by its continuance at last brought about a certain amount of paralysis in the nerves belonging specially to the vessels. And certainly there is nothing in the history of convulsion which is contradictory to this view of the relations between "irritation" and inflammation. For is it not true that the epilepsy may be present in its most violent forms where it is impossible to associate the "irritation" upon which the convulsion depends, with the faintest blush of inflammation in any part of the nervous system or elsewhere? And has it not just been seen that the epileptiform convulsion which may be associated with cerebral inflammation is anterior or posterior to this inflammation,—is, in fact, a substitute for rigor or subsultus?

91. *The condition of the function of innervation during convulsion is one which supports the notion, that the convulsion is connected with depressed and not with exalted action in the function.*

All the previous considerations lead to this conclusion, and to this conclusion only.

92. *The condition of the circulation and respiration during convulsion is one which necessitates the conclusion that convulsion is connected with depressed and not with exalted action of the nervous system.*

This conclusion is also inevitable, for it has been seen that the condition of the circulation and respiration during convulsion is incompatible with anything but the most depressed condition of vital action in the nervous system and in the system generally.

93. THE GENERAL CONCLUSION TO BE DEDUCED FROM THE CONSIDERATION OF THE CONDITION OF THE RESPIRATION, AND CIRCULATION, AND INNERVATION, DURING CONVULSION, IS THIS,—THAT THE PATHOLOGY OF CONVULSION IS AS MUCH IN HARMONY WITH THE VIEW OF MUSCULAR MOTION SET FORTH IN THESE LECTURES AS IT IS OUT OF HARMONY WITH THE CURRENT VIEW ON THIS SUBJECT,—THAT, IN FACT, CONVULSION IS THE SIGN OF DEPRESSED, AND NOT OF EXALTED, VITAL ACTION.

LECTURE VI.

2. ON THE THERAPEUTICS OF CONVULSION.

I spoke at some length upon this subject in the Gulstonian Lectures, which I had the honor of delivering in this College three years ago; and I do not propose to trespass upon your patience by recapitulating what I then said. Indeed, all I propose to do at present is to offer a few detached hints upon some points which seem to be of primary importance, and which have some claim to novelty.

94. *There is reason to believe that the diet in many cases of chronic convulsive disorder ought to contain somewhat more than an average quantity of oily and fatty matters, and somewhat less than an average quantity of lean meat.*

There is a common notion, not confined to non-medical persons, that lean butcher's meat is the one thing necessary to strengthen a weak system; and I believe that the carrying out of this notion not unfrequently complicates the difficulties which prevent the successful treatment of the case under consideration, and of many other cases also. With such a diet, as it seems to me, the blood is likely to get into a semi-gouty condition, unless there be a degree of activity in the circulation and respiration which is not likely to be met with in epilepsy, chorea, hysteria, or any other form of chronic convulsive disorder; and I have an impression that the undoubtedly beneficial influence of bromide of potassium in so many cases of epilepsy is owing, in part at least, to the fact that this salt, like iodide of potassium, corrects the semi-gouty condition of the blood arising from this and other causes. At any rate, I have no doubt as to the practical advantages of so regulating the diet in the cases under consideration as to diminish the usual allowance of fibrinous articles and to increase the usual allowance of oily and fatty articles. I have increased the quantity of the latter articles for the same reason as that which has led me to employ cod-liver oil in many of these cases, and of this I shall have to speak presently.

95. *There is reason to believe that gymnastic exercises are very beneficial in the great majority of chronic convulsive cases.*

During the last three or four years, I have seen several cases of epilepsy, chorea, and hysteria, in which undoubted good has resulted from the adoption of a regular course of suitable gymnastic exercises; and the more I see the more I am satisfied that a course of this kind is a very important adjuvant in the treatment of these and many other cases. I recall to mind more than one case of epilepsy in which the patient has said, that he has often warded off an attack which seemed to be imminent by a bout at the trapezium; and I have at present a case under treatment in which good seems to have been done by adopting a practice recommended by Dr. Henry Silvester, in the treatment of consumption,—a practice which may, perhaps, be brought under the head of gymnastics. Having ascertained that the mere dead weight of the arms hanging by the side reduces the amount of air which might be taken into the chest, to the extent of ten cubic inches or thereabouts, Dr. Silvester proposes that a phthisical person shall now and then eke out his insufficient respiration by breathing in such a manner as to get rid of this weight,—by breathing, that is to say, with the hands taking hold of something fixed at a sufficiently high level, or, what answers the purpose still more easily, with the hands clasped together and resting upon the top of the head. And this proposition appears to have much to recommend it, not only in phthisis, but also in other cases in which, as in epilepsy, the respiration is wanting in activity. I also think that a collateral argument in favor of gymnastics may be derived from Dr. Silvester's investigations upon artificial respiration, for these investigations show that as much as from nine to forty-four cubic inches of air may be made to pass in and out of the chest by merely pulling the arms upwards and then bringing them backwards to the sides, and that this movement of the arms is itself a mode of performing artificial respiration which is more effectual than any other. Of course, the beneficial influence of gymnastics is not confined to the respiratory function. On the contrary, this influence tells equally upon the circulatory and upon all other functions, as it indeed must do if it acts in this manner upon the respiration, for the activity of the respiration is a fair criterion of vital action generally.

96. *There is reason to believe that the action of cod-liver oil is beneficial in the great majority of cases of chronic convulsive disorder.*

For the last four years I have employed cod-liver oil in many

cases of epilepsy, chronic epileptiform disorder, chorea, and hysteria; and, so far as I can judge, I have no reason to be dissatisfied with the results. I can also refer to the experience of my friend and colleague, Dr. Anstie, as bearing out my own experience in this respect most fully. I was led to this practice, and also to that of recommending a fair amount of fatty and oily articles of diet, by remembering that fatty matter is, as is seen in the following table, (which gives the mean of six

	Infants.	Youths.	Adults.	Aged persons.	Idiots.
Fat,	3·45	5·30	6·10	5·32	5·
Phosphorus,	·80	1·65	1·80	1·09	0·85
Albumen,	7·	10·20	9·40	8·65	8·40
Osmazone and Salts,	5·96	8·59	10·19	12·18	14·82
Water,	82·79	74·26	72·51	73·65	70·93

analysis of the human brain, by M. L'Heretier,) as important ingredient of brain-tissue; and not of brain-tissue only, for it is found that the ingredients of the spinal cord and of the nerves generally are substantially the same. Remembering this fact, and remembering also the reasons which oblige me to believe that the function of innervation is carried on imperfectly in all convulsive maladies, I came to the conclusion that there might be a starved condition of the nerve-tissue, which might, perhaps, be remedied by cod-liver oil or by oily and fatty articles of food; and, be this theory right or wrong, I think, as I have said, that I have no reason to be dissatisfied with the results of putting it in practice.

97. *There is reason to believe that phosphorus is a suitable remedy in many cases of chronic convulsive disorder.*

For the last four years, I have used phosphorus in the majority of the cases of convulsion in which I have used cod-liver oil, and for the same reasons, and with the same results. I asked myself whether the fact set forth in the preceding table, (¶ 96,) that phosphorus is present in nerve-tissue, and that the amount of this ingredient seems to have some direct relation to the activity of the nervous function, being as much as two per cent in adult life, and below one per cent in infants and idiots, might not show that phosphorus is required as food by a weak nervous system,—as much required, perhaps, as is iron in cases where there is a deficiency of red corpuscles in the blood; and this question, once put, seemed to require an answer in the affirmative. "In small doses," says Dr. Pereira, "phosphorus excites the nervous, vascular, and secretary organs. It creates an

agreeable feeling of warmth in the epigastrium, increases the fulness and frequency of the pulse, augments the heat of the skin, heightens the mental activity and the muscular powers, and operates as a powerful sudorific and diuretic." In large doses, phosphorus, without doubt, is a caustic poison; in proper doses it produces the very changes which are necessary in epilepsy and in other cases of chronic convulsive disorder. In proper doses, and under the eye of a medical man, it is quite innocent of harm, and it may be productive of much good. This inference is that which may be drawn from what I said; and this inference, so far as I can see, is not contradicted by experience. Given in the large doses in which phosphorus has been given in a few cases already on record, the good resulting may have been doubtful, very doubtful; but this experience is nothing to the point, for there is no reasoning in any case as to the effects of medicinal doses from the effects of poisonous doses. Given in medicinal doses, I have seen enough to know that this remedy may be given, not only without harm, but with the unmistakable promise of real and substantial good. The form in which I first gave the phosphorus was the phosphorated oil of the Prussian Pharmacopœia, a preparation which is made by dissolving twelve grains of phosphorus in a fluid ounce of almond oil, by the aid of warm water. About four grains of the phosphorus is taken up, and the usual dose is from five to ten minims. I gave this oil always with cod-liver oil, in a little orange wine, twice or thrice a day. In many cases, however, this mixture proved to be so nauseous that the stomach refused to tolerate it; and lately I have often given the oil and the phosphorus separately, using the ethereal tincture of phosphorus of the French Codex as the vehicle for the phosphorus. I have given the oil with tolerable regularity so long as it seemed to be wanted, and the tincture now and then, especially when the symptoms called for a stimulant. I direct one fluid drachm of the ethereal tincture to be mixed with two fluid ounces of sulphuric ether, and preserved in a capped bottle, the dose being half a fluid drachm to one fluid drachm, mixed with water at the instant of swallowing it. In the ethereal tincture of phosphorus of the French Codex, four grains of phosphorus are dissolved in one fluid ounce of ether, and, consequently, the strength is the same as that of the phosphorated oil of the Prussian Pharmacopœia.

98. *There is reason to doubt the suitableness of belladonna as a remedy in many cases of epilepsy and other forms of chronic convulsive disorder.*

I have for some time been in the habit of looking upon the dilatation of the pupil, which results from the action of belladonna, as an objection to the employment of this medicine in epilepsy, and in cases more or less akin to epilepsy; and my chief reason for so doing has been a growing impression that the alterations in the size of the pupil are due to vascular rather than to muscular changes in the iris, and that these alterations furnish an exact index to the condition of the circulation within the brain. I have, in fact, been led to agree with those physiologists who look upon contraction of the pupil as being, in a great measure, due to a state of turgescence in the vessels of the iris, and upon dilatation of the pupil as being to the same extent the consequence of an empty state of those vessels. I have been led to do this, partly by thinking, as did Bichât that the pupil enlarges in the dark, not because there is then an end to the "irritation" which had kept certain sphincter fibres in a state of contraction so long as they are acted upon by the light, but because the *iris* has passed out of a state of expansion, which state was determined by the action of the light,—a view which brings the movements of the iris in the light and the movements of the cushions of the sensitive plant under similar circumstances into the same category; and partly by reflecting upon the history of five cases of acute cerebral meningitis which have been under my care during the last eighteen months. In these cases I saw the pupil contracted to its utmost limit when the symptoms denoted unequivocal active determination of blood to the head, and dilated before this state was developed and after it had passed off: and the more I pondered upon these phenomena, the more I became convinced that the size of the pupil was dependant upon the condition as to fulness or emptiness of the minute arteries and capillaries of the brain; that, in fact, the pupil became contracted because the vessels of the iris participated in the arterial turgescence of the brain, and that the pupil became dilated because the vessels of the iris participated in the emptiness of the cerebral vessels. And I was also confirmed in this opinion by remembering that the pupil contracts when the eye and the whole of the corresponding side of the head become injected with blood as the result of dividing the cervical sympathetic in that side, and that the pupil dilates when this vascular injection is made to pass off by galvanizing the divided sympathetic above the line of section. In the dilatation of the pupil, resulting from the action of belladonna, therefore, I have had what seemed to me a plain proof that this remedy produced an

anæmic condition of the brain, and an equally plain objection to the use of this remedy in epilepsy and in any other cerebral disorder in which there is reason to believe that the brain and nervous system generally are in an anæmic rather than in a hyperæmic condition; and I can say, without any hesitation, that I have seen no such unequivocal good from the practical employment of belladonna in these cases as to lead me to disregard this theoretical objection. Upon this point, however, I may not have seen enough to enable me to form a sound conclusion.

99. *There is reason to believe that opium may be a more suitable remedy than belladonna in some cases of epilepsy and in some other forms of convulsive disorder.*

Opium differs from belladonna in causing contraction of the pupil instead of dilatation; and, therefore, for the reasons set forth in the last paragraph, opium differs from belladonna in producing a hyperæmic instead of an anæmic condition of the brain, and in being suitable in cases in which belladonna is not suitable,—in cases where the brain is anæmic rather than hyperæmic,—in cases, it may be, of epilepsy, among others. As yet, however, I have met with few cases in which I have thought it expedient to test the correctness of this theory by putting it in practice.

100. *There is reason to doubt the efficacy of zinc as a remedy in epilepsy, and in cases akin to epilepsy.*

I am disposed to look upon zinc as having an action upon the system which is directly opposed to that of iron. Iron, as all know, has an astonishing power of favoring the nutrition and multiplication of the blood-corpuscles. Zinc, on the other hand, blanches the system, and induces, before long, the state which was once known as *tabes sicca*, and for which no other name has yet been devised. Zinc, indeed, exercises a peculiar desiccating influence upon the system. Dr. Pereira mentions the case of an epileptic gentleman who took daily, upon an average, twenty grains of oxide of zinc, until he had swallowed 3246 grains, and who at the end of this time was pale in the extreme, sallow, wasted away, almost idiotical, with the tongue thickly coated, the bowels constipated, the inferior limbs cold and œdematous, the abdomen tumid, the arms cold and shrivelled, the skin dry and almost like parchment, the pulse slow, thready, and scarcely perceptible. This patient experienced no change for the better in his fits; but he soon recovered from the effects of the zinc under appropriate treatment. I have also myself seen four cases, different from this one only in being not quite so severe.

and I have met with many cases in which the prolonged use of zinc, in one form or another, has produced decided sallowness and bloodlessness of the complexion. I find also that brass-founders, who are exposed to the fumes of deflagrating zinc, are often dried up and wizened in a curious manner. And Dr. Greenhow has recently shown, in addition, that these men are apt to suffer, particularly in the afternoons of the days spent in the casting shop, from what is called "brass-founders' ague," —a disease beginning with malaise, tightness or constriction in the chest, nausea, repeated rigors, and ending in profuse sweating after a short and faintly marked hot stage. Here, then, is evidence that zinc is capable of producing a form of convulsive malady,—for rigor is a form of such malady,—as well as of producing the *tabes sicca* which has been described. Here, indeed, is evidence which may, perhaps, throw some light upon the disorders of the nervous system, in which zinc is likely to do good or harm. That zinc does not always do good in these disorders is evident. My colleague, Dr. Marcet, who has for some time past given oxide of zinc very extensively in these disorders, reports very favorably of the result in some cases, but not in others. He does not report favorably of this mode of treatment in ordinary cases of epilepsy, and his report, I am satisfied, will not clash with the experience of the great majority of practitioners. Nay, it is a significant fact that M. Herpin, who has written a thick volume in praise of the virtues of oxide of zinc in epilepsy, has for some time been dissatisfied with his own favorite remedy, even to the extent of discarding it not unfrequently for a remedy which savors more strongly of the *materia medica* of the middle ages than that of the nineteenth century—poudre de Neufchâtel, or, in plain words, powder of fried mole. Dr. Marcet reports more favorably of the action of ozide of zinc in those cases in which he had to deal with various vague head symptoms, without convulsion,—a result which will also coincide with the experience of not a few. How then is this? Is it that the zinc does good in those cases of brain disorder in which there is a disposition to congestion of the brain, without any deficiency in the amount and quality of the blood, by virtue of that power which produces *tabes sicca* when pushed to an injurious extent? Is it that the zinc does good when the condition to be combated is hyperæmic, and harm when this condition is anæmic? These are questions to which I know no better answers than that which is contained in the preceding considerations, and this answer is so plain as to require no further comment to make it plainer.

101. *There is reason to believe that alcoholic stimulants are the most trustworthy antispasmodics in the prevention and treatment of convulsion.*

The wider experience of the last four years has not shaken my convictions upon this point. On the contrary, I do not remember any one case in which there was not something to strengthen these convictions. I have very recently seen a case of aggravated chorea in which there had been no sleep for five days and nights, and no cessation to the movements of any moment, in which a wineglass of port wine given every half-hour, with an egg beaten up in the alternate dose, produced quiet and sleep in ten hours, and in which a continuance of the same treatment, only in a less vigorous style, left the patient well, so far as the chorea was concerned, in a week; and I could cite other cases, at least three, to the same effect. I could also cite many cases of epileptic and hysteric convulsion averted, or its recurrence prevented, by means of a proper use of alcoholic stimulants.

102. *There is reason to believe that bloodletting, in one form or another, may be permitted in certain cases of convulsion, in order to prevent certain consequences of the convulsion.*

There is nothing in the pathology of convulsion to justify the notion that convulsion is likely to be prevented by bloodletting; but it is not difficult to understand that cases of epileptic or epileptiform convulsion may be met with in which the veins of the brain may be so gorged with black blood as to put the patient in imminent danger of apoplexy, and in which this danger may be somewhat lessened by abstracting a small quantity of blood. And, possibly, this may be no wrong practice in such a case. At present it is plain that past experience is too much disregarded in this matter. Why, it may well be asked, if moderate bloodletting be so serious a matter, should a woman require periodical bleeding to keep her in health? Nor must the astonishing power of multiplication belonging to the blood-corpuscles be lost sight of in this matter. Speaking of the rapidity with which an anæmic patient became convalescent in St. Mary's Hospital, Dr. Thos. King Chambers says,—and what he says requires no further comment,—"She weighs 8 stone, or 1792 oz.: of this 2-7, or 512 oz., is blood; and of this blood 133-1000, or 60 oz., should be red globules. Now, the analysis of MM. Andral and Gavarret show that in cases of anæmia of at all a marked character, (as this was,) we may expect at least three-fourths of the red blood-discs to disappear: so that when she came into the hospital it may fairly be assumed

that she did not possess more than 15 oz.; and now I think she may be assumed with equal fairness to have got up to 45 oz., which is conceding that she still wants a-quarter of perfect health. By this she must have made 20 oz. of red blood-discs, —that is the most important organic constitutent of upwards of 150 oz. of blood,—in a month.

103. THERAPEUTICALLY AS WELL AS PATHOLOGICALLY, THERE IS, IN FACT, EVERY REASON TO BELIEVE THAT THE MEANS TO BE EMPLOYED IN THE TREATMENT OF CONVULSION ARE THOSE WHICH EXALT, AND NOT THOSE WHICH DEPRESS, VITAL ACTION.

Editorial.

A TREATISE ON HYGIENE: WITH SPECIAL REFERENCE TO THE MILITARY SERVICE. By WILLIAM A. HAMMOND, M.D., Surgeon-General of U. S. Army; Fellow of the College of Physicians of Philadelphia, &c., &c., &c., &c. Philadelphia: J. B. LIPPINCOTT & Co. 1863.

This is a full-sized octavo volume of 604 pages, published in excellent style, and containing a treatise on a very important branch of medical science, by one who occupies a high official position. After a short preface and introduction, the work is divided into three sections, as follows:—

Section 1st.—On the examination of recruits.

Section 2d.—Of the agents inherent in the organism which affect the hygienic condition of man.

Section 3d.—Of agents external to the organism which act upon the health of man.

The work is illustrated by 74 cuts, representing chiefly plans for hospitals and apparatus connected therewith. The first section, which occupies about forty pages, we shall pass by with the single remark, that it contains plain, practical, and generally judicious directions in relation to the examination of recruits: pointing out plainly the items of qualification and disqualification to be observed by the medical examiner. The next section contains ten chapters on the following subjects:—1. Race; 2. Temperaments in general; 3. Particular temperaments; 4. Idiosyncrasy; 5. Age; 6. Sex; 7. Hereditary tendency; 8.

Habit; 9. Morbid habits; 10. Constitution. These ten chapters simply present a brief summary of such facts and opinions relating to the several topics as are familiar to every reading man in the profession. Together, they occupy eighty-six pages of the work.

Section III. contains twenty-nine chapters as follows:—1. The atmosphere; 2. The accidental or non-essential constituents of the atmosphere; 3. Physical properties of the atmosphere; 4. Temperature; 5. Light; 6. Electricity; 7. Water; 8. Soil; 9. Locality; 10. Climate; 11. Acclimation; 12. Habitations; 13. Hospitals; 14. Principles of hospital construction; Field hospitals; 16. Lighting of hospitals; 17. Heating of hospitals; 18. Ventilation of hospitals; 19. Barracks; 20. Camps; 21. Food; 22. Alimentary principles; 23. Physiological and sanitary relations of food; 24. Animal compound aliments; 25. Vegetable compound aliments; 26. Accessory food; 27. Alimentation of the soldier; 28. Clothing; 29. The hygienic relations of clothing with the several parts of the body.

It will be seen that six of these chapters, filling more than one hundred and forty pages, are devoted to the consideration of hospitals and their management, and constituting, by far, the most valuable part of the work. The remaining chapters, except the 26th, like those in the second section, present only brief summaries of the most common-place and familiar facts in relation to each topic. For instance, in the chapter on Temperature, instead of a philosophical explanation of the effects of caloric on living animal structures, and the best means of obviating the consequences of its excess or deficiency, we have reproduced the old and familiar story of the excursion of "Sir Joseph Banks, Dr. Solander, and nine others;" and the usual allusion to the destruction of Napoleon's army on the frozen plains of Russia. We are also told that the direct rays of the sun, in hot weather, are liable to produce "sun-stroke," and that a wet cloth worn on top of the head is a good prophylactic; but in what "sun-stroke" consists, pathologically, is not so much as hinted at. The only chapter in which the author attempts to enter upon a philosophical discussion of the subject

under consideration, is that which treats of "Accessory Food." Under this head, the author includes condiments, such as pepper, mustard, vinegar; drinks,—as alcohol and its compounds; tea and coffee; and tobacco. The comments on the three first articles are brief and unexceptional. Those in relation to alcohol and its compounds are of a widely different character. He introduces the subject with the following very modest declaration, namely:—"The chief reason why the advocates of a total prohibition of the employment of alcoholic liquors have been unable to carry conviction to those to whom they have addressed themselves is, that their remarks have *mainly consisted of invectives*, and that whatever facts they have brought forward have been altogether based upon the *immoderate* use of the agents in question." For fifty years, this has been the uniform reply of the advocates of alcoholic liquors as beverages, to all arguments, whether founded on statistical facts, showing the relative power of physical endurance between those who use and those who do not use alcohol; the relative ratio of sickness and mortality; or on direct physiological experiments. To show by actual results of labor in every department of human toil, whether in the harvest-field, the workshop, the brickyard, the army, or the navy; whether in summer or in winter; whether under the burning rays of a torrid sun, or amidst the icebergs of the arctic regions; that those who use alcoholic beverages, whether fermented or distilled, actually do a less average amount of labor, are capable of less physical endurance, and suffer a higher ratio of attacks of sickness than those who, under exactly the same circumstances, wholly abstain from such liquors, is to deal *mainly* in "invectives," is it? To prove, by direct experiment, that alcohol enters the blood unchanged, disturbs and perverts the sensibility and action of the nervous structures, depresses the elementary properties of all the tissues,—thereby retarding organic changes,—and disturbing the natural play of those affinities, by which nutrition, disintegration, and secretion are effected; and is finally evolved again from the economy as alcohol, from the lungs, kidneys, &c., is also dealing mainly in "invectives," we suppose.

To show by the physiological action of alcohol, and by innumerable cases, taken from every rank of human society, that the habitual *moderate* use of alcoholic drinks leads, in nine cases out of every ten, to the "immoderate" use of the same, is also dealing in "invectives." Well, be it so for the present. Immediately following the paragraph quoted above, our author proceeds as follows:—"No one can for a moment deny that alcoholic liquors, when used in excessive amount, are not only injurious to the individual, but are also in the highest degree pernicious to society. * * * But are such facts to influence us against the *proper* use of all beverages which contain alcohol. * * * Do we reject mutton because some one has killed himself by eating too heartily of mutton-chops?"

We certainly should not condemn the proper use of an article merely because its abuse produced injurious effects. But we cannot help asking, whether the W. A. HAMMOND, M.D., author of the work before us, ever heard of a certain Order, No. 6, excluding calomel from the supply-table of the army, *because it had been used* "immoderately" by some of the army surgeons, issued by W. A. HAMMOND, M.D., Surgeon-General of United States Army?

Without wasting words, however, on minor matters, let us first see what are the actual effects of alcohol on the human system, as shown by the experiments and researches of Drs. PERCY, PROUT, BÖCKER, LELLEMAND, HAMMOND himself, and others, as collated in the chapter under consideration. They may be summed up as follows:—

1st.—The alcohol taken into the stomach is rapidly absorbed into the blood, circulated with it throughout the whole system, and is eliminated chiefly through the lungs and kidneys; being readily detected, by the proper tests, both in the vapor of the breath and in the urine.

2d.—While in the blood, it produces an exhilerating effect upon the brain and nervous centres, causing thereby disturbance in the mental operations and sensibilities of the patient.

3d.—Its presence in the blood diminishes the aggregate amount of eliminations from the several excretory organs of the

body, doubtless by diminishing both structural disintegration and secretion.

These propositions may be considered as well settled by a great variety of experiments and observations, both in Europe and America. But the practical inferences to be drawn from them are still the subjects of much controversy. Thus, a class of chemico-physiologists, embracing LIEBIG, MOLESCHOTT, HAMMOND—the author of the work before us, and many others, claim that, because the presence of alcohol in the human system diminishes the aggregate amount of eliminations, and causes an increase in the weight of the body, provided the digestion of other food goes on as usual, it actually supplies the place of food.

Thus, on page 539 of the work before us, Dr. HAMMOND says:—"We have seen that it takes the place of food, and that the weight of the body increases under its use. Any substance which produces the effects which we have seen to attend on the use of alcohol, even though it is not demonstrable at present that it undergoes convertion into tissue, is food;" and on the next page we find a quotation from MOLESCHOTT, as follows:—"Alcohol is a savings-bank for the tissues,—if the expression will be understood. He who eats little and drinks moderately of alcohol, retains as much in his blood and tissues as he who, in corresponding relations, eats more and drinks neither beer, nor wine, nor brandy." It is on this assumption, that alcohol is a substitute for food, that our author and others of the same physiological school base nearly all their reasoning in favor of the general use of alcoholic compounds as beverages. It is mainly on this same assumption that a very large class of medical writers and practitioners base their recommendation of alcoholic drinks in the treatment of numerous important diseases. Hence the question, whether this assumption or inference is correct; whether it legitimately follows from the premises or facts proved, is one of the highest importance in its relations to physiology, therapeutics, and social life. Plainly and concisely stated, the premises and the inference is as follows:—

1st.—It is definitely proved, by a great variety of experiments, that under the influence of alcohol, other things being equal, the sum total of the excretions or eliminations from the lungs, skin, kidneys, bowels, &c., are diminished, and the body gains in weight. This is the premise from which it is inferred.

2d.—That such diminution of elimination is caused by the alcohol retarding the natural disintegration of the tissues, while the processes of assimilation and construction of tissue is allowed to continue; and that thereby the alcohol acts the part of actual food.

Thus, the naked question is evolved: whether a retardation of the natural disintegration of tissue and elimination of the resulting effete matter is actually equivalent to, or will, physiologically, compensate for, a certain amount of assimilation and nutrition? Dr. HAMMOND, and the advocates of the use of alcoholic beverages, generally assume the affirmative; but is their position in consonance with the known and acknowledged laws which govern the nutrition and disintegration of living tissues? It is universally conceded, that living organized animal structures are composed of organic atoms or cells, none of which remain permanent, but each of which serves its purpose and gives place to a new one. It is further generally conceded, that the performance of every functional act or display of force, whether mental or physical, is attended by more or less displacement of these atoms. It is this constant displacement of old organic atoms that renders a regular supply of food or ingesta necessary to the maintainance of life in all the higher orders of animals. This constant atomic change, being a physiological law of the organization, taking place under the guidance of an inherent vital affinity, cannot be retarded, except by directly impairing or weakening the affinity itself, or by introducing some new agent possessing a stronger affinity for some of the atoms composing the tissues than is possessed by oxygen or whatever naturally effects the primary steps of disintegration. To do either of these, manifestly induces a *pathological* condition incompatible with the continuance of health. For every intelligent physiologist knows, that

on the constant display of vital affinity in the organic or atomic changes taking place in the tissues, depends the development of caloric to maintain animal temperature; the generation of nerve sensibility; the elaboration of the secretions; and, indeed, all the distinctive phenomena of animal life. Hence, whatever agent introduced into the blood, in a healthy state of the system, is capable of retarding the process of disintegration, must, if persisted in, necessarily produce either disease, or perverted nutrition, or both. Dr. HAMMOND himself sees, at least partially, this conclusion, as is evident from the following, from page 540:—"Alcohol retards the destruction of tissue. By this destruction force is generated, muscles contract, thoughts are developed, organs secrete and excrete. Food supplies the material for new tissue. *Now, as alcohol stops the full tide of this decay, it is very plain that it must furnish the force which is developed after it is ingested. How it does this is not clear.*"

Here is a full acknowledgment of the fact: that to retard the metamorphosis or disintegration of the tissues is to retard, in the same ratio, all the force producing processes or organic functions of animal life. True, he attempts to escape from the dilemma by supposing that the alcohol itself comes to supply the force which its presence prevents the natural atomic changes in the tissues from generating: but how it does this, he frankly confesses, "is not clear."

His attempt, on the next page, to explain the matter by saying, "it is not at all improbable that alcohol itself furnishes the *force* directly, by entering into combination with the products of tissue decay, whereby they are again formed into tissue, without being excreted as urea, uric acid, &c.;" is not only destitute of proof, but, unfortunately, in direct conflict with the results of a well-devised series of experiments by LALLEMAND, PERRIN, and DUROY, which show that all the alcohol taken, is ultimately eliminated through the excretory organs unchanged. The same is confirmed by Dr. HAMMOND'S own experiments, during which the alcohol was generally detected both in the breath and the urine some time after it was absorbed from the stomach.

Thus, turn whichever way they will, those who advocate the doctrine, that a retardation of disintegration is equivalent to nutrition, involve themselves in difficulty. Indeed, the proposition itself is a physiological absurdity. If it were true, it would only be necessary to find some substance that would arrest the processes of tissue disintegration entirely, and we might live on without the necessity or *expense* of eating at all. Indeed, if alcohol is capable of retarding tissue destruction, and, at the same time, of furnishing the required *force* itself, what is to hinder a man from living on it indefinitely? Whether fascinated by the beautiful theory of HAMMOND or not, many a poor fellow has practically tested it, but has generally been unfortunate enough to have the experiment cut short by a fit of *delirium tremens*. We have thus far pursued the subject as though the experiments cited by our author actually proved that alcohol retarded the disintegration of tissues; but they really prove no such thing. They simply show, that, while alcohol is present in the system, the sum total of eliminations is diminished and the weight of the body increased, provided the usual supply of ordinary food has been continued. Whether the diminished excretions are owing to retarded disintegration of tissues or to the direct action of alcohol on the excretory organs, whereby their power to perform their respective functions becomes impaired, is a question open for discussion; and one to which we may recur at some future time.

The only remaining ground on which alcohol can be claimed as food, in any sense, is that originally put forth by LIEBIG, and quoted approvingly by our author, namely, that, like other hydro-carbonaceous substances, it furnishes material for respiration. This, however, is also directly disproved by all the experiments cited in the chapter before us; by a series of experiments performed by myself, and reported to the Annual Meeting of the American Medical Association, in 1851; and by a great variety of facts derived from observation. It is evident that alcohol can act as respiratory food only by entering into combination with the oxygen; the resulting products of which would be carbonic acid gas and water; and, as a con-

sequence, we should have an abundance of carbonic acid exhaled from the lungs and an increased temperature of the system. Whereas the experiments cited by our author all show a diminution of carbonic acid in the exhaled air and the presence of alcohol unchanged; while those reported by myself, show a positive decrease of temperature while under the influence of that agent. There are several other items in this chapter we had intended to notice, but time will not permit at present. We have written enough to show the utter fallacy of the main positions of the author; in his effort to convert what he acknowledges in the outset to be a "violent poison," into beneficial food and drink. That part of the chapter commending the use of tobacco, is founded on the same fallacies and exhibits the same absurdities.

Indeed, his direct definition of food, necessarily destroys all distinction between aliments and poisons,—between food and medicine. After stating that the presence of alcohol in the system diminishes the destruction of tissue and causes the body to increase in weight, he says:—"Any substance which produces the effects which we have seen to attend on the use of alcohol, even though it is not demonstrable at present that it undergoes conversion into tissue, *is food.*" Now, it is well known that opium, and, probably, all the other narcotics, diminish the organic changes; and, if taken in moderate doses, allowing appetite and digestion to continue, the body increases in weight. The same results have been known to accompany the use of arsenic and other acknowledged poisons. Would Dr. HAMMOND, therefore, call opium and arsenic food? To do so would only be equalled in absurdity by the following remark of LIEBIG, gravely quoted as authority by our author:—"The use of spirits is not the cause, but an *effect of poverty.* It is an exception to the rule when a *well-fed* man becomes a spirit-drinker." Such an assertion, in this country, where a loaf of bread costs but a trifle more than a glass of whiskey or a mug of beer, and where men are almost as frequently seen staggering in broad-cloth as in rags, will scarcely produce any other effect than to excite a smile at the theoretical vagaries of men

eminent in some departments of science. It is hardly necessary to say, in conclusion, that the examination of this work of Dr. HAMMOND has exceedingly disappointed us. The publishers have done their part of the work well; but, so far as it claims to be a treatise on hygiene, it is very superficial and abounding in most mischievous errors.

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